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I. *The Radio-Balance. A Thermoelectric Balance for the Absolute Measurement of Radiation, with Applications to Radium and its Emanation.* *By* H. L. CALLENDAR, *M.A., LL.D., F.R.S., Professor of Physics at the Imperial College of Science and Technology, S.W.*

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1. *Introductory.*—Electric compensation methods, in which the radiation to be measured is directly balanced or compensated by means of heat supplied by an electric current under steady conditions, possess many advantages over the more simple calorimetric methods, which involve the observation of the rate of rise of temperature in a receiver of known thermal capacity. In a report on the measurement of solar radiation presented to the British Association in 1900 (Brit. Assoc. Report, 1900, p. 36) I discussed the general principles of the calorimetric method, and described a form of instrument, termed a radio-calorimeter, specially designed for investigating the errors of the method. Although the method itself was comparatively simple, the observations were tedious and troublesome if a high order of accuracy was attempted, and it was necessary to take account of the effects of lag in the rate of rise of temperature, even when the receiver was a small disc of copper, and the rise of temperature was observed by means of a very sensitive thermo-junction. The chief advantages of electric compensation methods are that they are practically unaffected by lag, since the temperature conditions are steady, and that they are independent of measurements of thermal capacity, which are generally somewhat uncertain.

The best known electric compensation methods are probably those of Angström and Kurlbaum. Angström's pyrheliometer contains a pair of similar strips, each 20 mm. long and 1·5 mm. wide, one of which is exposed to radiation while the other is heated to the same temperature by an electric current. Equality of temperature is indicated by a sensitive galvanometer connected to a pair of copper-constantan thermo-junctions, attached to the backs of the receiving strips, but insulated from them by thin silk paper and shellac varnish. The rate of heat absorption H in watts per square centimetre is given by the simple formula, $H=C^2r/b$, where C is the balancing current in amperes, r the mean resistance per centimetre of the strip in ohms, and *b* the mean breadth in centimetres. Small differences between the strips are eliminated by exposing either alternately to the radiation to be measured while the other is heated by the current. A small correction is required for the variation of *r* with temperature. Allowance should also be made for the fact that the temperature of the strip exceeds that indicated by the thermometer, but this correction will not usually amount to more than 2 or 3 parts in 1,000. It is necessary to assume that the resistance of the strip is uniform, since the temperatures are balanced at the centres only. Unfortunately, it is a difficult matter to measure the resistance of so delicate and short a strip satisfactorily, or to verify its uniformity or constancy at any time. The other fundamental measurement, that of the width of the strip *b*, is also difficult to make on account of the raw edge of smoke black. Owing to the thinness of the strip, about 95 per cent. of the final deflection is produced in the first minute after exposure to radiation, but owing to loss of heat by conduction at the ends of the strip, the temperature at the centre does not become steady to 1 in 1,000 for more than five minutes. The final approach to a steady temperature may be tested most accurately by heating the strip with a constant current, and balancing the E.M.F. of the thermo-couple on a potentiometer. Unfortunately, the instrument cannot be relied on to remain constant in its indications for considerable periods. This want of constancy is most readily detected by comparing the indications of the two strips under the same conditions at different times. In one instrument tested, the difference between the strips increased from 7 to 25 per cent. in the course of two years. Similar differences have been recorded by other observers, and appear usually to be caused by the splitting or separation of the compound strip. A probable source of error is the deterioration of the smoke-black film, the absorption coefficient of which is given as 98 per cent. for a new instrument; but this could hardly account for such large differences between the strips. Perhaps it is too much to expect that so delicate a compound strip, consisting of two different kinds of metallic foil stuck together with shellac and silk paper, should remain constant for many years; but constancy is one of the chief desiderata in an instrument for absolute measurement.

The method of Kurlbaum can be applied to any sensitive bolometer of suitable construction, and avoids many of the difficulties of Angström's method, but introduces others which make it less convenient for measurements of solar radiation, to which it has not generally been applied. The method consists in observing the deflection of the galvanometer when the bolometer of resistance R is exposed to the radiation to be measured, and is traversed by a current C_1. The radiation is then cut off, and the current increased to a value C_2 giving the same deflection. The total heat AH absorbed from the radiation in watts is given by the formula,

$$AH=(C_2^2-C_1^2)RC_2/C_1.$$

where A is the effective area of the bolometer.

The whole surface of the bolometer may be of the order of 10 sq. cm.; and the difficulty of measuring the width of the strips is avoided by using two grids, the strips of the second being behind the spaces of the first. There is no difficulty in measuring the resistance R at any time, and the grid can be electrolytically coated with platinum black, which is more permanent and more accurately reproducible than smoke-black. On the other hand, it is necessary to balance the bolometer against manganin resistances which are not appreciably affected by the current, and it is therefore impossible, during the absolute measurement, to compensate for changes in the surrounding temperature in the usual way by balancing the bolometer exposed to radiation against a precisely similar grid. The whole change of resistance observed by Kurlbaum was only 3 in 10,000 of R, corresponding to a rise of temperature of 0·1°C. In order to obtain an accuracy of 1 in 1,000 it would be necessary that the temperature of the bolometer enclosure should not change by 0·0001°C. during the observation. In Angström's method the compensation for change in the surrounding temperature is extremely perfect if the strips are equal. In addition to possible errors from change in temperature, the most important sources of error in the bolometric method appear to be (1) the rearward grid of the bolometer is partly shielded from radiation by the front grid, but is equally heated by the current. This will not affect the absolute measurement, provided that the front and back strips are of the same dimensions and resistance, but if they are not equal, the error will be proportional to the shielding and to the difference of resistance. Otherwise uniformity of resistance of the strips in each grid is not so important as uniformity of resistance in the strips of the Angström instrument, because want of uniformity will introduce errors of the second order only in the case of the bolometer, whereas in the Angström instrument the resulting errors may be of the first order, if the resistance of the strip at the centre, where the thermo-junction is attached, differs from the mean value assumed. (2) The area of the bolometer surface which is exposed to radiation may not exactly coincide with that which is heated by the current. This would not matter except near the ends of the strips where they are cooled by conduction, and where radiation and current would differently affect the connections and supports. This end effect is shown by a gradual increase or creep of the deflection due to warming of the end connections and supports, when the bolometer is continuously exposed. The rates of creep are generally different for current and radiation, and some uncertainty is thus introduced into the absolute measurement.

2. *Absolute Recording Bolometer.*[*]

The gradual creep of the zero due to the warming of the end connections and supports is most troublesome when a bolometer mounted on a support of slate or other badly conducting material is continuously exposed to radiation. In order to avoid this source of trouble in the recording bolometer designed for taking records during the total eclipse of Aug. 30th, 1905, a method of compensation was employed which I have adopted in the construction of platinum thermometers for many years. Two similar grids were constructed with strips of different lengths, as indicated in the diagram (Fig. 1) on opposite page. These were similarly mounted side by side, and were connected in opposite arms of a Wheatstone bridge of the type employed in platinum thermometry. The end effects were the same for both grids, and were automatically eliminated. The resistance measured corresponded to the central 2 cm. only of the grid with the longer strips. The effective area of the radiation admitted was determined by a water-cooled diaphragm 2 cm. wide with a sharp edge, corresponding approximately with the difference of area of the grids. This makes the effective area of the bolometer definite, and completely eliminates creep due to the end effect even when the bolometer is exposed all day to full sunshine. For continuous recording, the difference of resistance between the grids is balanced against an equal resistance of the same platinum strip enclosed in the same water-jacket, in order to eliminate the effect of small changes of temperature in the water-jacket. To measure the heating effect of the current, and obtain the absolute equivalent of the radiation, the balancing resistance of platinum strip R is replaced by a constant resistance S, by shifting a plug, and the resistance is balanced without changing the current. The radiation is then cut off, and the current is altered to give the same heating effect as the radiation, under the same conditions. The creep due to the heating effect of the current on the end connections being eliminated, there is

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[*] The absolute recording bolometer was described and exhibited at a meeting of the Physical Society of London in May, 1905. Publication of the paper was delayed in order to include a reference to the results of the eclipse observations in August. Unfortunately, the manuscript of the paper and all the original observations were lost, together with other luggage, on the way to the observing station in Spain. The paper itself was never printed, although a record of the reading appears in the "Proceedings" of the Physical Society. It did not seem worth while at the time to repeat all the observations and rewrite the paper, because a general account of the instrument and the results obtained during the eclipse were published in the Eclipse Report, in the "Proceedings" of the Royal Society for October, 1905. Moreover, the experiments on the Radio-Balance had already progressed so far as to show that it would prove to be a more convenient instrument in practice. It appeared desirable, however, to include in the present paper a brief account of points not mentioned in the Eclipse Report, in order to explain the difficulties of the method, and to show why it was ultimately abandoned in favour of the radio-balance.

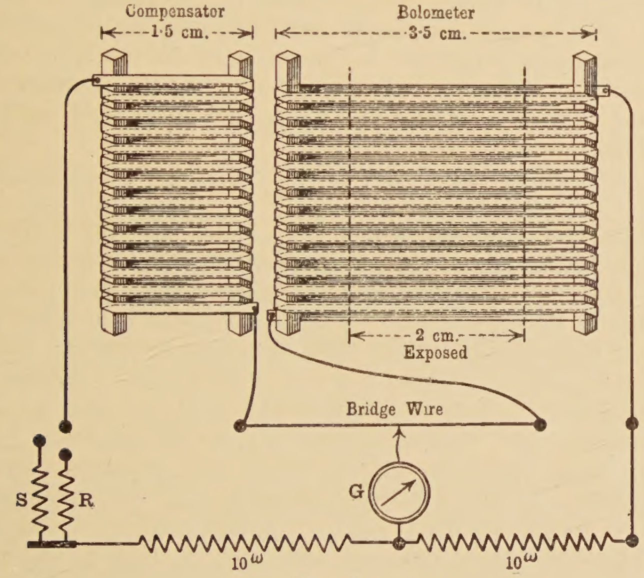


Fig. 1.

no need of hurry, and it is better to balance the resistance in place of employing the deflection method of Kurlbaum. We thus obtain the accurate formula,

$$AH=(C_2^2-C_1^2)R, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where A is the area of the aperture, H the intensity of the radiation absorbed in watts per square centimetre, R the observed resistance (*i.e.*, the difference of resistance of the grids) when the current C_1 is flowing with radiation, or the current C_2 without radiation. Kurlbaum's formula requires the additional factor C_2/C_1, to allow for the change of deflection with current, and neglects the fact that the resistance of the bolometer is different in the two cases for the same deflection. This difference was negligible in his observations, but could not be neglected in applying the method to solar radiation.

An incidental advantage of the end-effect compensation is that it is unnecessary to employ the excessivley thin and fragile Wollaston foil, which is generally used for bolometer grids, but which is difficult to obtain perfectly uniform and free from flaws. Quickness nearly equal to that of the galvanometer is readily obtained by means of rolled wire strip, provided that the end effects are compensated, with the advantage of greater uniformity and ease of adjustment, and with less risk of strain or injury in coating with platinum black or other manipulations. The instrument above described was made with rolled wire strip, carefully tested for uniformity, and was adjusted and tested for compensation over the range 0° to 100°C. by putting steam and ice cold water through the jacket.

The chief objections to the bolometric method in this form are (1) that the determination of the radiation in terms of the electric current requires a separate observation which cannot easily be made simultaneously with the observation of the radiation by a null method. It is therefore necessary to use a water jacket to keep the conditions fairly steady. (2) That the method involves the observation of small changes of resistance, which necessitates the use of somewhat elaborate apparatus, and special attention to details of compensation. In order to meet the objection that the observations of radiation and equivalent heating current in the bolometric method above described are not simultaneous, and to provide some compensation for changes in the surrounding temperature which need not be disconnected during the absolute measurement, I devised and tested various other modifications of the bolometric method, two of which, the four-grid bolometer method and the method of the bright and black bolometer, are of so distinct a type as to be deserving of mention.

3. *Four-Grid Bolometer Method.*

This is a fairly simple method, which gives very good compensation for changes in the surrounding conditions, but in which the end-effects are not exactly compensated. I devised it specially to suit one of Lummer and Kurlbaum's standard pattern bolometers, in which the grids are symmetrically arranged in pairs, the strips of the second grid being behind the interspaces of the first, and those of the third behind the interspaces of the fourth. As usually connected, the first and second grids are in opposite arms of a Wheatstone bridge, of which the third and fourth grids form the other two arms. For the application of the four-grid method, the connections are altered so that the first and second are in parallel, in series with the third and fourth in series (or vice versa, the first and second in series, in series with the third and fourth in parallel). The ratio of the first and second in parallel to the third and fourth in series is approximately 1 to 4, and is balanced by resistances of about 1,000 and 4,000 ohms, forming the other two arms of a bridge. The heating effect of the current C_1 on 3 and 4 in series, is four times as great as on 1 and 2 in parallel. Grids 1 and 2 are exposed to radiation, and the current increased to C_2, till the balance is restored. The connections are then changed to 3 and 4 parallel, and 1 and 2 series; 3 and 4 are exposed to radiation, and the current adjusted to balance as before. This is necessary in order to eliminate small differences between the grids. In each case the actual resistances are measured by taking the ratio to a standard 10 ohm coil in series with the grids, but are not required to more than about 1 in 1,000, the limit of accuracy of the radiation balance. If R is the resistance of each separate grid, C_1 the small current for setting the ratio, C_2 the increased current for balancing the radiation, A the exposed area, a the absorption coefficient, and H the radiation in watts/square centimetre. The approximate equation for the absolute value of the radiation is,

$$aAH = (C_2^2 - C_1^2)3R/2, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This equation neglects the small differences between the grids, and the small changes of resistance with heating current, which are easily taken into account in the actual measurement. The currents are measured by the P.D. on the 10-ohm standard. The most important point is to adjust the current C_2 to give the same value of the ratio with radiation, as the current C_1 without.

4. *Bright and Black Bolometer Method.*

A pair of bolometers of equal dimensions and resistance, but one bright and the other black, as indicated in the diagram, Fig. 2, are simultaneously and symmetrically exposed to the radiation to be measured. The difference of temperature between them will be approximately proportional to the intensity of the radiation, and may be utilised for recording the radiation, as in my usual pattern of sunshine recorder. The bright and black bolometers are connected in series with two equal and constant resistances S and S′, of the same order of magnitude as the bolometers, by means of which they can be balanced to equality. They are also connected in parallel with two high resistances P and Q, by means of which the ratio of either bolometer to S or S′ may be determined. If, now, the current through the bolometers is increased while they are exposed to radiation, their resistances may be brought back to equality, and the corresponding rise of temperature deduced from the ratio P/Q. Assuming that the loss of heat by conduction and convection is the same for both bolometers at the same temperature, and that the ratio of the emissive power of either to that of a black body is equal to its coefficient of absorption, the rise of temperature observed when the

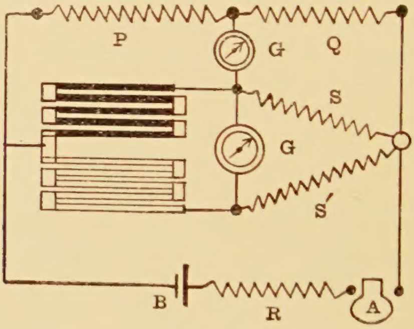


Fig. 2.

resistances are balanced is the same as the rise of temperature of a perfectly black body under the same conditions of exposure to radiation. The intensity of the radiation to be measured can therefore be calculated from the known value of the radiation constant. The function of the electric current in this method is to compensate for the loss of heat by conduction and convection, and the value of the current itself is not required. The rise of temperature observed is the same as it would be in the absence of the current if conduction and convection were also absent. The method would be theoretically perfect if the theoretical conditions assumed were accurately satisfied (as they are no doubt approximately), and if it were practically possible to construct two exactly similar bolometers of equal resistance, one bright and the other black. The theoretical difficulty arises from the fact that the radiation to be measured is generally of a different wave-length and quality from that emitted by the bolometer, and that the coefficient of absorption of a bright metallic surface may be appreciably different for different wave-lengths. The correction for this difference must be separately determined, and gives rise to an uncertainty of the same order of magnitude as the determination of the coefficient of absorption of a single black bolometer. Against the practical difficulty of constructing two similar bolometers may be set the advantage of excellent compensation for end-effects and for changes in the surrounding temperature. When carefully applied the method gives results which agree very well with other methods, but have no great claim to superior accuracy. Like most bolometric methods depending on the observation of small changes of resistance, it is too difficult and delicate for general use. The method works best for low temperature radiation, as from a source at 100°C., since the absorptive and emissive powers of the grids are then very nearly proportional; and the correction is much less important than for a source at a high temperature.

A general objection to all bolometric methods is that on account of the large surface exposed it is necessary to take account of the angular aperture of the receiver as well as the radiant. Even in the case of solar radiation, a bolometer 2 cm. or 3 cm. square, unless provided with a tube of unwieldy length, will include a large and uncertain area of the sky. The difficulty of securing similar exposure is often serious in comparing instruments of different types, especially when one of the receiving surfaces is square or rectangular, like that of a bolometer. On the other hand, the quantity of energy received and available for measurement increases in direct proportion to the surface, so that it is often advantageous to employ a bolometric method for weak sources when a large area is available.

5. *The Disc Radio-Balance.*

The disc form of radio-balance was a natural development of the radio-calorimeter described in the British Association Report, 1900. The latter instrument had a thick copper disc 1·3 cm. diameter, supported by two fine iron-constantan thermo-couples at right angles, either of which could be used for observing the rate of rise of temperature of the disc when exposed to radiation. It occurred to me that by passing an electric current through one of the thermo-couples, the radiation might be directly compensated by the heat absorption due to the peltier effect. The troublesome effects of lag could thus be eliminated, and the radiation deduced in absolute measure from the peltier effect, without a knowledge of the thermal capacity of the disc. The disc was made smaller and thinner, 3 mm. to 4 mm. diameter, and 0·3 to 0·5 mm. thick, to secure greater sensitiveness and uniformity of temperature; and the radiation aperture was reduced to 2 mm. diameter and placed about 5 mm. in front of the disc. The intensity of radiation which can be directly compensated in this manner by the heat absorption due to the peltier effect with a given instrument is limited in practice by the fact that the current also generates heat C^2R in the wires of the couple, where R is the effective resistance of the couple through which the current is passed. If a is the absorption coefficient of the blackened surface of the disc,

A the area of the aperture admitting radiation in square centimetres.

H the intensity of the radiation in watts per square centimetre,

P the coefficient of the peltier effect in volts,

C the current through the couple in amperes,

$C_0 = P/R$, the neutral current, giving neither heating nor cooling,

when the radiation is directly compensated by the current, we have the equation

$$a\mathrm{AH} = \mathrm{PC} - \mathrm{C}^2\mathrm{R} = \mathrm{PC}(1 - \mathrm{C}/\mathrm{C}_0). \quad . \quad . \quad . \quad . \quad (3)$$

The value of the coefficient P is deduced from the observed thermoelectric power of the couple $dE/d\theta$, by means of the well-known relation $P = \theta(dE/d\theta)$, where θ is the absolute temperature. The value of the neutral current C_0 can easily be observed at any time with an accuracy of 1 or 2 parts in 1,000, which is more than ample, since this term is relatively small. The most difficult measurement is the area of the aperture A; and the most uncertain factor, the absorption coefficient, a.

In order to compensate accurately for changes in the surrounding temperature, and to avoid the necessity for a water-jacket, two similar discs with similar connections were mounted side by side in a hollow copper cylinder, 5 mm. thick. The connections for such a pair of discs are shown in the diagram, Fig. 3. The discs of this particular instrument were 3 mm. diameter, and the current for heating or cooling either disc was carried by four pairs of constantan-iron wires, 0·2 mm. diameter, in order to reduce the effective resistance. Either of these could be connected by a double-pole switch to the circuit consisting of battery B, rheostat R, and milliammeter A, for supplying, regulating, and measuring the current. The difference of temperature between the discs was indicated by the sensitive galvanometer G, connected to a single iron-constantan couple with one junction in either disc. The value of the peltier coefficient was 0·01493 volt at 15°C. The values of the neutral currents for the two discs were 0·791 and 0·793 ampere respectively, at 15°C. Taking $a = 1$, and $A = 0{\cdot}0320$ sq. cm., the current required to compensate radiation of intensity 1·0 calorie per square centimetre per minute, or 0·070 watt

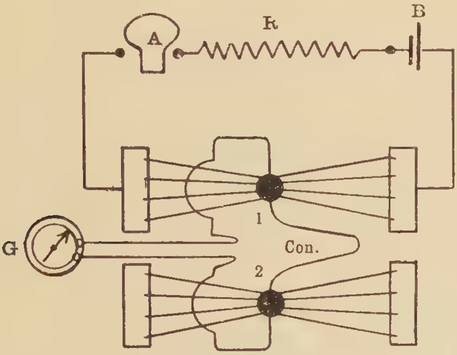


FIG. 3.

per square centimetre, is approximately C=0·200 ampere, which is about a quarter of the neutral current. The maximum radiation which can be directly compensated by the cooling effect occurs when C is half the neutral current, and is H=0·0925 watt per square centimetre, or 1·32 calorie per square centimetre per minute. The range could be extended if desired by reducing the resistance R, or the size of the aperture A. But the above range was suitable, and sufficient for most purposes with direct sunshine. With a sensitive galvanometer of the suspended coil type a deflection of the order of 500 mm. to 1,000 mm. at 1 metre for 1 calorie per square centimetre per minute could be obtained. Several different combinations of discs and wires were tried. With one pair of wires in place of four in parallel for the current the effective resistance was increased four times, and the range of direct compensation reduced to one quarter. But the sensitiveness was nearly doubled, so that this combination was more suitable for weak sources of radiation. The accuracy of the theoretical formula (3) was carefully verified by measuring both the current and the E.M.F. of the couple on a potentiometer, over the whole range up to the neutral current. The method was also tested at various temperatures up to 100°C. by means of a water-jacket. The temperature coefficient of the neutral current with the particular couple employed was found to be very nearly the same as that of the peltier effect. The coefficient of C^2 in equation (3) was thus practically independent of the temperature.

6. *The Radio-Balance as a Pyrheliometer.*

Fig. 4 shows the radio-balance arranged as a pyrheliometer. The thick copper cylinder containing the discs and their connections is extended by a tube containing suitable diaphragms for limiting the exposure. The tube is mounted equatorially, so that the sun can be followed by a single worm wheel on the polar axis. The whole is mounted on a levelling stand, with level and compass for the approximate adjustment of the polar axis. The tube is gilt in place of being lacquered. The gilt stands exposure to sunshine better than lacquer, and also greatly diminishes the absorption of heat. There are no external terminals, which may be a source of trouble, especially in an instrument which has to be exposed to full sunshine. Connections are made to the measuring apparatus by means of three pairs of flexible leads, one foi the galvano-

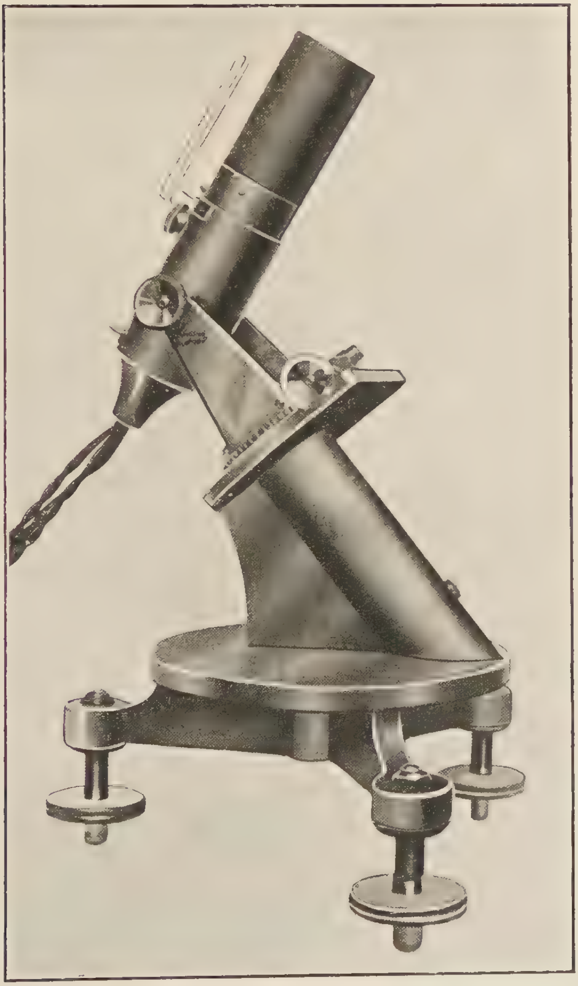


Fig. 4.

To face p. 12.

meter, and one for each of the two discs. The latter are connected to the double pole switch for conveying the current to either disc. The leads are permanently connected to the couples inside the copper cylinder, and are seen in the figure issuing through an ebonite thimble at the base of the tube.

The chief advantage of the direct compensation method, especially when the radiation to be measured is variable, like sunshine, is that the result is obtained by a single observation on one disc, and that it is unnecessary to upset the conditions and adjustments by interchanging the discs. A subsidiary advantage, which is quite appreciable when a portable milliammeter is used for measuring the current, is that a very open current scale may be obtained in the neighbourhood of the radiation to be measured. Thus, with the constants already given we obtain the following scale for the radio-balance, taking $a=0{\cdot}98$:—

Current in amperes	0·050	0·100	0·150	0·200	0·250	0·300	0·350
Radiation, cals/cm.^{2}	0·312	0·581	0·811	0·995	1·140	1·242	1·300
„ C^2 scale	0·063	0·250	0·563	1·000	1·563	2·250	3·063

An error of 1 per cent. in the current between 200 and 300 milliamperes is equivalent to an error of only one-half of 1 per cent. in the radiation. Whereas on the C^2 scale of the bolometer or Angström pyrheliometer, an error of 1 per cent. in the current would be equivalent to an error of 2 per cent. in the radiation. Since few portable milliammeters are correct to more than one half of 1 per cent., it is a distinct gain to be able to make the accuracy of this measurement four times less important.

When the intensity of solar radiation is rapidly variable, as is often the case near London, even when the sky appears to be quite clear, the most convenient method of observation is to set the current at a suitable constant value, such as 0·200 ampere, corresponding to nearly 1 calorie per square centimetre per minute, and to observe the deflections of the galvanometer, which are proportional to the changes in the intensity of the radiation. The scale of the galvanometer must in this case be tested under the conditions of the experiment, preferably by reversing a known small current C through the disc, and observing the corresponding deflection D on reversal, which is equivalent to a heat supply of 2 PC.

A disadvantage of the radio-balance is that the increase of the peltier effect per 1°C. for the couples employed is very nearly 0·0001 volt in 0·0150 volt. It is necessary to read the temperature to 0·1°C. (by means of the attached thermometer shown in Fig. 4, the bulb of which is inside the copper cylinder), in order to obtain an accuracy of the order of 1 in 1,500. For this reason the instrument should be allowed to attain an approximately steady temperature after exposure to sunshine, before observations are taken.

The radio-balance may also be employed in the same manner as the Angström pyrheliometer, by exposing one of the discs to radiation, and heating (instead of cooling) the other disc by the current. The range of compensation in this case is unlimited, since the joule effect acts in the same direction as the peltier effect. It is merely necessary to change the negative sign in equation (3) to positive. The disadvantage of this method is that errors in the current measurement become more important, and that it is necessary to interchange the discs, as in the Angström method, in order to eliminate small differences between them, which, however, should not amount to more than 1 per cent.

It is an advantage that the black coating on the disc, which is liable to deteriorate by exposure, can readily be renewed at any time without risk of injury to the couples. The discs may be cleaned with spirit, and coated with a thin film of black shellac varnish. They are then smoked, the wires being protected by a card with holes through which the discs protrude. The double film of varnish and smoke-black adheres better than smoke-black alone, and gives a higher absorption coefficient, approximately 99 per cent. for sunshine. To recoat the delicate strips of a bolometer, or of an Angström pyrheliometer is a far more difficult and hazardous undertaking.

The most obvious error possible in the construction of the disc radio-balance is that, if the wires of the couple connected to the galvanometer are not symmetrically situated with respect to the wires conveying the current through the disc, there will be a difference of potential introduced into the galvanometer circuit, which will be equivalent to an error in the peltier effect. The possible limits of this error can be tested by connecting the two constantan wires to the battery and the two iron wires to the galvanometer. This arrangement eliminates the peltier effect, but makes the flow of current through the disc such as to produce the greatest possible disturbance in the galvanometer circuit. All the discs were tested in this way, and it was found that the galvanometer deflection was only 5 to 6 per cent. in this extreme case of the deflection due to the peltier effect when the flow was symmetrical. The possible error when the wires were connected in the usual way for the peltier effect could be estimated from this observation, and did not amount in any case to more than 1 in 1,000.

The fundamental assumption involved in the method is that the temperature of the copper disc is practically uniform, owing to its small size and high thermal conductivity. Several different methods were tried of connecting the wires to the discs, but without appreciable variations in the results. The method finally adopted, as being the simplest, was to punch with a fine needle a number of holes equidistant from the centre of the disc and evenly spaced. The ends of the iron and constantan wires were inserted in these holes and simultaneously sweated in place by floating a small quantity of solder over the back of the disc. Discs prepared in this way gave very uniform results, and were easily reproducible with a high degree of accuracy.

Owing to the relatively large mass of the copper disc, as compared with a bolometer film which may be a hundred times thinner, the first movement of the galvanometer, when the disc is exposed to radiation without simultaneously applying the balancing current, is not so rapid as in the case of a bolometer for an equal final deflection. But owing to the absence of end-effects, and to the perfect compensation for changes in the surrounding temperature, the radio-balance reaches its final steady deflection to 1 in 1,000, quite as quickly as the bolometer, and in most cases with greater precision and regularity.

7. *The Cup Radio-Balance.*

The disc radio-balance is comparatively easy to construct and to reproduce, and is very suitable for observations of solar radiation, but cannot be applied to measurements other than radiation. The sensitiveness is limited to that of a single thermo-couple, and the coefficient of absorption of the disc is an uncertain factor, though probably less uncertain than in most other methods. In order to diminish the uncertainty of the coefficient of absorption, the receiving surface was made in the form of a cup, 3 mm. to 4 mm. diameter and 8 mm. to 10 mm. deep. It was found that, if the interior of the cup was not blackened, but left as it came from the spinning, its effective coefficient of absorption was 94 to 96 per cent. as against 30 per cent. for a flat disc in a similar condition. It was a fair inference that if the interior of the cup were blackened, its coefficient of absorption would be practically equal to 100 per cent. In order to increase the sensitiveness, the cup was mounted in a tubular pile, as indicated in the sectional diagram, Fig. 5. The upper junctions of the pile were bound firmly round the middle of the cup, from which they were insulated by thin paper and shellac. The

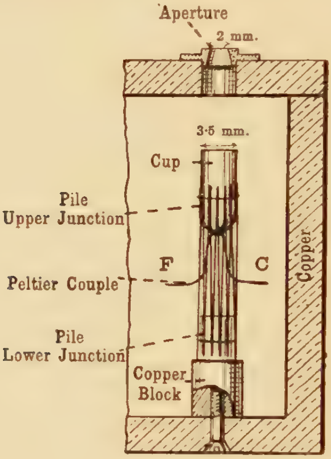


Fig. 5.

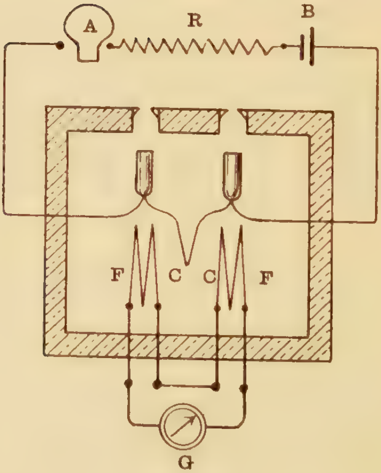


Fig. 6.

lower junctions were similarly fastened to a small copper block, screwed to the base of the thick copper cylinder, in which a second similar pile and cup was enclosed to secure the necessary compensation for changes in the surrounding conditions. This arrangement is indicated in the diagram of connections, Fig. 6, in which, however, the cups are shown separated from the piles, in order to facilitate the tracing of the connections, which are the same as for the disc radio-balance, except that the piles are insulated from the cups. The peltier couples for compensating the radiation are single couples soldered to the bottoms of the cups. The four pairs of leads for the two couples and the two piles are brought out separately, so that the insulation can be readily tested at any time. The piles in most of the instruments constructed on this plan consisted of 12 couples each, and gave about seven times the sensitiveness of a single couple. This could easily be tested by exposing one of the cups to radiation and observing the deflections (1) when the piles were connected in the usual way, (2) when the single peltier couple was connected to the galvanometer in place of the piles.

8. *Conditions of Sensitiveness.*

It may be of interest to give the theory on which the construction of the piles was based, as it has often proved useful in similar cases in the design of thermoelectric apparatus. The heat supply W in watts required to raise the temperature of the cup 1°C. above its surroundings, is equal to the rate of heat loss a from the cup by radiation and convection (including conduction loss through the peltier couple) per degree rise of temperature, together with the rate of loss by conduction through the wires of the pile itself. The latter loss will be directly proportional to the number of couples n, and inversely proportional to the resistance R of each couple, so that it may be represented by a term of the form bn/R. The deflection D of the galvanometer, of resistance G, per degree rise of temperature of the cup, will be directly proportional to the number of couples n, and inversely proportional to the whole resistance $\mathrm{G}+n\mathrm{R}$. The watts per scale division of deflection W/D, which is to be a minimum, may be written,

$$\mathrm{W/D}=(a/n+b/\mathrm{R})(\mathrm{G}+n\mathrm{R})=a\mathrm{G}/n+bn+b\mathrm{G}/\mathrm{R}+a\mathrm{R},$$

This will evidently be a minimum when $n=\sqrt{a\mathrm{G}/b}$, and $\mathrm{R}=\sqrt{b\mathrm{G}/a}$, which gives the appropriate number of couples and the resistance of each. The dimensions of the wires within reasonable limits are immaterial, provided that the resistance is the same. The above conditions are equivalent to $G=nR$, and $a=bn, R$; or the whole resistance of the pile should be equal to that of the galvanometer, and the loss of heat by conduction through the pile should be equal to the sum of the other losses from the cup. A preliminary estimate of a and b gave 12 as a suitable number of couples in each pile, employing the same iron-constantan wires 0·2 mm. diameter of suitable length, with a total resistance of 4·8 ohms and a 5 ohm galvanometer. Other piles with different wires and different numbers of couples were subsequently made in order to test the theory, but it was found that the preliminary estimate was so nearly correct that no advantage was gained in sensitiveness, although the experiments were useful as throwing a good deal of light on other details of construction. Thus it was found to be important to mount the lower junctions of each pile on a copper block fastened to the cylinder, and not on an insulating block, as might appear better and simpler at first sight. In the latter case the lower junctions are heated by conduction along the pile; the deflection reaches a maximum in about a minute and then diminishes slightly; and the final deflection is smaller and less regular than with the copper block mounting, owing to the retention of heat by the insulating block.

9. *Method of Using the Cup Radio-Balance.*

The cup radio-balance may be used for measuring radiation in the same way as the disc radio-balance, by heating or cooling either cup separately by means of the peltier couples, the piles in which the cups are mounted being oppositely connected in the galvanometer circuit so as to indicate the difference of temperature between the cups. In this method the equation previously given applies, the neutral current must be determined, and the range of direct compensation by the cooling effect is limited. Since, however, the piles are necessarily insulated from the cups, it is more difficult to secure exact equality of sensitiveness with the piles than with the single couples soldered to the discs of the disc pattern, and it is desirable to interchange the cups at each observation. For this reason, and also to secure greater range and exact elimination of the joule effect, it is more convenient with the cup balance to proceed in a different way. The peltier couples are connected in opposition, and the same current is passed through both, so that the cup exposed to radiation is cooled while the other is heated by the peltier effect. (This cannot be done with the disc balance because the galvanometer couple is soldered to the discs.) The current is adjusted to give an approximate balance. The other cup is then exposed to radiation, and the current simultaneously reversed without altering its value. The sum of the opposite deflections (or the algebraic difference of the deflections) gives the defect of balance, which can always be made very small, and is easily reduced to watts, since the sensitiveness of the galvanometer s in scale divisions per watt is readily determined at any time, and remains very nearly constant. If D', D'' are the deflections of the galvanometer when cups 1 and 2 respectively are exposed, and the radiation is balanced by a current C through both peltier couples, the equation for the radiation H is

$$AH = 2PC + (D' - D'')/s, \qquad \ldots\ldots \quad (4)$$

A is the mean of the areas of the two apertures in front of the two cups. $D' - D''$ is the movement of the spot of light in scale divisions measured in the same direction as the movement produced by switching over radiation from cup 1 to cup 2. If the current is too large the movement will be in the opposite direction, and $D' - D''$ will be negative. The sensitiveness s is found by observing the deflection d due to reversal of a small current c through the peltier couples, which gives $s = d/2Pc$. Since it is not immediately obvious that the above method simultaneously eliminates the joule effect and compensates for inequality between the piles, the following more general investigation is given :—

10. *Elimination of the Joule Effect and Inequality of Piles.*

If W', W'' are heat sources in watts in cups 1 and 2 capable of being interchanged,

P', P'', coefficients of the peltier effect for couples soldered to cups 1 and 2,

R', R'', effective resistances of peltier couples for cups 1 and 2.

$P = (P' + P'')/2 =$ mean of peltier coefficients.

s', s'', deflections in scale divisions per watt supplied to cups 1 and 2.

$s = s' + s'' =$ total deflection per watt transferred from cup 1 to cup 2.

D' observed deflection with W' in 1 and W'' in 2, balanced by a current C cooling 1 and heating 2, measured in the direction due to heat in 1.

D'' the corresponding deflection measured in the same direction when W'' and W' are interchanged, and the same current C is reversed.

Collecting the quantities of heat supplied to either cup, we have

$$s'(W'-CP'+C^2R')-s''(W''+CP''+C^2R'')=D'.$$
$$s'(W''+CP'+C^2R')-s''(W'-CP''+C^2R'')=D''.$$

Taking the difference between these two equations, the joule effects are exactly eliminated even if s', s'', and R', R'', are widely different, provided that the current C is unaltered on reversal. We thus obtain

$$(s'+s'')(W'-W'')-2C(P's'+P''s'')=D'-D''.$$

Since P', P'' are always very nearly equal, and s', s'' rarely differ by so much as 2 or 3 per cent., we may neglect the product of the two small quantities $(s'-s'')$ and $(P'-P'')/2$, and write $(P's'+P''s'')=Ps$, which gives the equation,

$$W'-W''=2PC+(D'-D'')/s.$$

The two piles must always be connected in opposition in the galvanometer circuit in order to eliminate the effect of external disturbances. It is important for this reason that the piles should be nearly equal. The values of s', s'' are not required separately, but their equality may be tested by reversing the same current C through each of the peltier couples separately, and comparing the corresponding deflections, which are proportional to $P's'$, and $P''s''$. It may be assumed that P' and P'' are very nearly equal, but these cannot be tested without using a separate source of heat as explained in the next section. It is, of course, most important to test the insulation of each pile from the copper cylinder, and from the peltier couples soldered to the cups. This test can be made very easily at any time.

11. *Verification of the Peltier Effect.*

A special advantage of the cup form of radio-balance over the disc form is that the actual value of the peltier effect can be verified for either couple at any time after the apparatus has been made by placing in the cups the two junctions of a standard couple, the electromotive power of which has been directly determined. The junctions of the standard couple are attached to small blocks or cups of copper closely fitting the cups of the radio-balance. The deflection due to the standard couple should be very nearly the same as that due to the couples of the balance, if the wires for both couples are from the same reels. In making the test one junction must be inserted in each cup in order to compensate for external disturbances due to conduction of heat along the wires. The standard couples must be in place, but disconnected from each other, when the deflection due to the peltier couples of the balance is observed. The cups can be tested separately, for equality of P' and P'' by reversing the current through one of the junctions only, but both junctions must be in place. The equality of s' and s'' can be similarly tested. If any difference is observed bewteen P' and P'' it is generally due to defective soldering, or bad insulation. Such differences might arise from want of uniformity of the constantan wire. If the wire is not uniform it is no good for thermoelectric work.

The radio-balance is well suited for measuring the peltier effect for any other thermo-junction by comparison with the standard, or for verifying the fundamental relation $P=\theta(dE/d\theta)$. This may be most simply effected by using a small resistance coil as a source of heat in one of the cups, and balancing the heat generated by means of the peltier couples; but it is most essential to insert in the other cup a pair of compensating leads similar to those of the resistance coil and traversed by the same current, in order to balance the heat generated in the leads, and the conduction along the leads. The compensating leads should also have a dummy coil wound on them in order to balance the thermal capacities. A still more accurate method is to use a thermo-couple for the leads of the resistance coil and the compensating leads, and to find the current required to balance the joule effect in the coil against the peltier effects in the leads. The result is then compared with the thermoelectric power of the couple as directly measured for the same combination.

I have tried various other modifications of the same method, and find there is no great difficulty in verifying the relation to nearly 1 in 1,000 with the radio-balance. It would be very difficult to obtain anything like this order of accuracy with a calorimetric method, unless a similar balance or compensation method were employed. In Jahn's observations with a Bunsen ice calorimeter (" Wied. Ann." 34, p. 755, 1888) the peltier effect was only a small fraction of the total effect observed, and the discrepancies between theory and observation were of the order of 20 per cent. With the radio-balance, the joule effect and conduction of heat along the wires are only a small fraction, about 5 per cent. of the peltier effect, and are accurately compensated. The observation of the peltier effect can be made in 10 minutes, and is more easy and accurate than the observation of the thermoelectric power.

12. *Method of Taking Observations.*

One of the chief difficulties in comparing different instruments for measuring radiation is that of securing a constant and reproducible source of radiation. For this purpose I employed the same 90-volt 100 c.p. focus lamps as were used in the investigation of the radio-calorimeter (B.A. Report, 1900). These give a fairly intense radiant of about 2 cm. square area, and have remained extremely constant, being generally run at between 70 and 80 volts. The voltage was adjusted either by means of a Weston laboratory standard voltmeter, accurate to about 1 in 1,000, or by an electric recorder working on the potentiometer method with a scale of 2 cm. per volt. The current was supplied by a large storage battery, and the radiation generally became constant in about 10 minutes. Owing to the irregular shape of the filament of the lamp, the distance from the mean plane of the filament could not be measured nearer than about 1 mm., but by keeping the lamp fixed in position the distances of the different instruments to be compared from the lamp could be adjusted to about 0·1 mm. by the aid of a fixed telescope. With 80 volts on the lamp an intensity of nearly 1 calorie per square centimetre per minute could be obtained at a distance of 20 cm. At this distance a variation of 1 mm. in the distance would be equivalent to an error of 1 per cent. in the radiation, so that it was necessary to adjust the distance to 0·1 mm. A more serious difficulty was that a variation of 1 per cent. in the voltage produced a variation of nearly 3 per cent. in the radiation, so that the radiation could not be reproduced with certainty to less than 2 or 3 in 1,000.

In the earlier experiments in 1905-6, a very sensitive galvanometer with a suspended magnet and mirror system was employed for observing or balancing the E.M.F. of the thermocouple due to difference of temperature between the discs.

Owing to the excessive sensitiveness of the galvanometer, and to magnetic disturbances due to electric railways, &c., in the vicinity, the E.M.F. of the couple could not be observed by the deflection method, and it was necessary to balance it on a special form of potentiometer of the type described in the Eclipse Report (*loc. cit.*). The current through the peltier couples was also measured by observing the potential difference on a standard resistance with the same potentiometer, which could readily be adjusted to suit a range of either 50 microvolts for the couple or 50 millivolts for the current. Owing to increasing magnetic disturbances due to the electrification of the Metropolitan and District Railways, it became necessary to employ galvanometers of the suspended coil type. Two of these, specially designed for thermo-electric work, with resistances of 13 and 7 ohms respectively, giving deflections of the order of 10 mm. to 20 mm. at 1 metre per microvolt, were obtained from the Instrument Co., Cambridge. A separate potentiometer, designed by the same makers, with a range of 30 millivolts reading to 1 microvolt, was used for the peltier currents, as it was found to be inconvenient to use one galvanometer and potentiometer for both purposes. A standard 1 ohm coil was used for currents up to 30 milliamperes, and a standard 0·1 ohm coil for currents from 30 to 300 milliamperes. Currents down to 3 milliamperes could be measured with an order of accuracy never less than 1 in 3,000. Currents above 300 milliamperes were seldom required, but were usually measured with a Weston laboratory standard ammeter. The galvanometer deflections on the balancing thermo-couples or piles could be read without difficulty to 0·1 mm., corresponding to 0·01 microvolt or less. Measured in terms of rate of heat supply, the sensitiveness obtainable with the moving coil galvanometers was about 1 to 2 scale divisions per microwatt for the cup radio-balances, and 2 or 3 microwatts per scale division for the disc balances. In the majority of the observations the heat supply was balanced by means of the current through the peltier couples, so that the residual deflection to be observed was usually very small. But if the deflection did not exceed 250 mm. on either side of zero, it was found that an accuracy of 1 in 1,000 could be obtained by observing the deflections, provided that readings were taken in a regular order at regular intervals. This was often useful as a check on the working of the apparatus.

Of the many hundreds of observations taken in testing the apparatus, it will suffice to give two or three specimens in order to illustrate different methods of observation and comparison. The first illustration is a comparison of two different disc balances B and C. with an Angström pyrheliometer at the same distance from the focus lamp at approximately the same voltage. Radio-balance B was a laboratory pattern instrument with water jackets, and had discs 3 mm. in diameter. Radio-balance C was mounted as a pyrheliometer, the discs were 4 mm. in diameter, and the wires were differently arranged, so that the values of the neutral currents were different for B and C. Observations were taken by deflection on radiation and current separately, as well as by direct balance. In the balance method, equation (3) applies, and it is necessary to know the neutral current, since the joule effect involves a correction of 10 to 15 per cent. In the deflection method, readings are taken with the current direct and reversed through either disc. The deflections on either side are unequal, but the joule effect is eliminated from the algebraic difference.

The values of the thermoelectric power $dE/d\theta$ in microvolts per 1°C., and of the peltier coefficient P in microvolts, were as follows for the samples of wire employed :—

Temp. C.	0°	5°	10°	15°	20°	25°	30°	35°	40°
$dE/d\theta$...	49·60	50·34	51·09	51·83	52·58	53·32	54·07	54·81	55·56
P.........	13,550	14,000	14,465	14,930	15,410	15,897	16,390	16,890	17,397

The currents C in the following comparisons are stated in milliamperes, and the heat supply W in microwatts, as being the most convenient units. The corresponding values of the radiation intensity H in watts per square centimetre are deduced by dividing W by the areas of the apertures in square centimetres and are reduced to calories per square centimetre per minute by the factor 60/4·18.

Radio-Balance B. Mean aperture 0·03031 *sq. cm. Neutral current* 0·804 *amp. at* 22°*C.*

—	Balance method.		Deflection with current alone.				Radiation alone.	
Disc. exp. ...	(1)	(2)	(1)	(1)	(2)	(2)	(1)	(2)
Temp. C.......	22·1°	22·4°	20·8°	20·7°	20·6°	20·6°	20·9°	21·3°
Volts	74·65	74·75	—	—	—	—	74·74	74·70
Current	−92·03	+92·00	−79·07	+78·95	−78·88	+78·83	—	—
Deflection ...	− 1·1	− 1·3	−187·0	+227·4	−226·8	+180·6	+219·0	−209·0
Microwatts .	1,270	1,277	1,223	414·4 sc.	1,220	407.4sc.	428·0 sc. = 1,272 mw.	

Mean result :—Radiation at 74·72 volts = 1,273 mw. = 0·04200 w/cm.² = 0·6029 cal./cm.² min.

A positive sign for current or deflection corresponds to evolution of heat in disc (1) or absorption of heat in disc (2), a negative sign the reverse.

Radio-Balance C. *Mean aperture* 0·03211 *sq. cm.* *Neutral Current* 0·702 *and* 0·698 *amp.*

—	Balance method.		Deflections with current alone.				Radiation alone.	
Disc exp.	(1)	(2)	(1)	(1)	(2)	(2)	(2)	(1)
Temp. C.......	22·0°	22·3°	22·4°	22·3°	22·0°	21·7°	22·3°	22·5°
Volts	74·64	74·58	—	—	—	—	74·65	74·55
Current	−99·92	+99·85	−79·48	+79·40	−79·30	+79·26	—	—
Deflection ...	− 9·2	− 9·6	−192·2	+224·8	−243·0	+177·6	−235·0	+217·3
Microwatts .	1,338	1,339	1,242=417·0 sc.		1,236=420·6 sc.		452·3 sc.=1,338 mw.	

Mean result:—Radiation at 74·60 volts=1,338·5 mw.=0·04168 w/cm.^{2}=0·5982 cal./cm.^{2} min.
Reduced to 75·0 volts B gives 0·6098 cal./cm.^{2} min. C gives 0·6078 cal./cm.^{2} min.

The difference is within the limits of error of the voltage, but might equally be due to a slight difference in the coefficients of absorption.

Angström Pyrheliometer. $H=7{\cdot}17\ C^2$ (*C in amps.*) *Smoke-black assumed* 98 *per cent.*

—	Radiation alone.		Balance method.		Current alone.	
Strip Exp.	(1)	(2)	(1)	(2)	(1)	(2)
Temp. C.	22·3°	22·4°	22·2°	22·4°	22·5°	22·3°
Volts	74·8	74·6	74·6	74·5	—	—
Current m.a. ...	—	—	−289·0	+255·7	+255·2	−288·7
Deflection	+243·0	−228·5	−2·5	−2·0	+229·0	−252·5
Cal./cm.^{2} min. ...	0·494	0·541	0·599	0·469	D/C^2=3,530	3,030

In using the Angström pyrheliometer by the balance method in the usual way, when strip (1) is exposed, the current is passed through strip (2), and vice versa. This tends to exaggerate any difference between the strips, and gives in the above example results differing by 24 per cent. The difference is partly due to a difference of sensitiveness of the thermo-junctions, caused by a slight difference in the attachment to the strips. This difference may be eliminated by comparing the deflection produced by the radiation with the deflection produced by the current *through the same strip*, which gives the more concordant results shown in the columns headed "radiation alone"; but this method cannot be used to obtain a balance. Both results are much too low, and it seems probable that the resistance of the strips has increased and the film of smoke-black deteriorated. The strips were not visibly split or separated, but an almost imperceptible defect of this kind generally reduces the readings considerably. The current deflections showed a marked tendency to creep, but were within 1 per cent. of their final values at the end of three minutes when the readings were taken.

With the cup radio-balance, since the cups were insulated from the piles, it was possible either to compensate the radiation by the heat absorption due to a negative current in the peltier couple soldered to the exposed cup (current through one cup only), or to pass the same current through both cups, cooling the cup exposed to radiation and heating the other to balance. Both methods are illustrated in the following comparison, which also illustrates the effect of blacking the insides of the cups.

Radio-Balance D. Neutral Current 0·380 *amp. Mean aperture* 0·3199 *sq. cm.*

—	Cups clean.				Cups blackened.			
	Current through exposed cup.		Both cups.		Both cups.		Current alone.	
Cup. exp. ...	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Temp. C. ...	19·62	19·68	20·18	20·22	19·58	19·62	19·68	19·70
Volts	75·72	75·68	75·45	75·35	75·20	75·10	—	—
Current	−103·52	+103·50	−37·55	−37·51	−38·35	+38·61	+ 8·060	−8·058
Deflection ...	− 4·2	− 4·8	− 6·3	− 8·3	−11·5	− 2·3	+88·5	−106·5
Microwatts .	1,172 at 75·70		1,155 at 75·40		1,199 at 75·15		195sc.=248mw.	
At 75 volts...	1,139 mw.		1,137 mw.		1,192 mw.=0·03725 watts/sq.cm.			

The ratio 1,138 1,192 of the results with the cups clean and blackened respectively gives 96 per cent. as the effective absorption of the clean cups. A similar experiment with radio-balance C (disc pattern), gave 26 per cent. as the effective absorption of the discs when clean but not polished. When coated with thick indian ink black, the coefficient of absorption of the disc was 94 per cent. With thin black shellac varnish, opaque but rather shiny, the absorption was only 91 per cent. When the shellac varnish was coated with smoke, which gave a very intense dead black, the absorption rose to 99 per cent. as tested by comparison with radio-balance D, assuming total absorption for the blackened cups. The disc radio-balance B, similarly varnished and smoked, also gave nearly the same result when compared with D.

Readings were taken at the same distance under the same conditions with the compensated bolometer with water in the jackets at 17·70°C.

Difference of bolometer and compensator $R_1=9{\cdot}0390$ ohms, with $C_1=38{\cdot}03$ m.a.

Radiation from lamp at 75·05 volts, $R_2=9{\cdot}2841$ ohms, with $C_1=38{\cdot}01$ m.a.

Screened from radiation, but current increased to balance at R_2, $C_2=114{\cdot}56$ m.a.

Exposed area 3·002 square cm. Radiation $=0{\cdot}03612$ watt per square centimetre.

Compared with the above value given by radio-balance D, this result would make the absorption of the platinum black on the bolometer slightly less than 97 per cent., which is not improbable, as the black was rather old and had probably deteriorated.

13. *Application to Radium and its Emanation.*

The cup form of radio-balance is very suitable for measuring small quantities of heat, such as that evolved by a milligram of radium. The specimen first tested was an old sample of impure radium bromide, supposed to be about 5 milligrams, which had been obtained about five years previously. It was sealed in a glass tube 3 mm. in diameter, and 4 cm. long. This could easily be transferred from one cup to the other, and the deflection observed, or compensated by the peltier couples. After several readings had been taken with fairly consistent results, giving 300 microwatts as the rate of development of heat, the sample was sent by post to Prof. Rutherford, who kindly undertook to compare it with his standards. He found the sample equivalent to 3·40 milligrams of pure anhydrous radium bromide, or to 1·99 milligrams of radium. On its return, the heating effect of the sample was found to have fallen to 267 microwatts, but it regained its old value in less than a month. These changes were subsequently traced to the escape of some of the emanation from the radium bromide, which was found to be not quite dry. Any emanation which escaped would diffuse through the tube, so that its heating effect would be lost to the small cup at the bottom. The cylindrical tube was unsuitable for an accurate determination on account of the relatively large loss of heat by conduction along the tube. The greater part of this radium sample was subsequently enclosed in a small glass bulb more nearly fitting the cup of the radio-balance, and the values of the heating effect, when the sample had recovered from the de-emanation, were found to be extremely constant.

Prof. Rutherford also very kindly prepared and sent two samples of emanation sealed in small glass bulbs fitting the cups of the balance. The first of these, by comparison with his standards, corresponded with the equilibrium amount from 25·8 milligrams of $RaBr_2$ at noon on May 31st. This value was verified on its arrival by comparison with the radium sample already tested by Prof. Rutherford. The heating effect of the emanation was tested daily for a fortnight, and at longer intervals subsequently, and was found to decay at the same rate as the -ray activity, falling to half value in 3·86 days. The mean deviation of the observations from the theoretical curve was only 0·2 per cent. The relative values of the heating effect could be measured in the early stages without difficulty to 1 in 1,000, but the accuracy attainable began to diminish when the heating effect had fallen to 200 microwatts.

The second sample of emanation arrived on July 7th, and was employed to demonstrate the heating effect of the emanation with the radio-balance at the meeting of the Physical Society on July 8th. Its heating effect at that time was 1,300 microwatts, equivalent to a deflection of 1,000 mm. at 1 metre with radio-balance E and the 7 ohm galvanometer. The value of this sample was given by Prof. Rutherford as being equivalent in γ-ray activity to 35·2 milligrams of $RaBr_2$ at noon on July 6th. It was compared on July 7th, 8th and 9th with the old sample of radium, which had remained sealed in its original glass tube. Assuming the radium to be equivalent to 3·40 milligrams of $RaBr_2$, the comparisons with the electroscope by means of the γ-rays agreed in making the emanation equivalent to 30·6 milligrams of $RaBr_2$ at noon on July 6th. A comparison of the heating effect observed on the same days with that of the first sample of emanation, assumed equivalent to 25·8 milligrams of $RaBr_2$ on May 31st at noon, made the value of the second sample equivalent to 30·4 milligrams of $RaBr_2$ at noon on July 6th. The origin of the discrepancy between Rutherford's value 35·2 for the second sample of emanation, and the values 30·4 and 30·6 obtained here by comparison with the first sample and with the radium, cannot now be traced, and tends to throw some doubt on the absolute values deduced for the heating effect of the radium and the emanation.

In order to obtain a more accurate comparison of the relative values of the heating effect of the radium and the emanation, the greater part of the radium was sealed, on July 9th, in a small glass bulb of the same size as the bulb containing the emanation. The radium was dried and almost completely de-emanated in the process, owing to the small size of the bulb. Its heating effect fell to a minimum of 71·3 microwatts in about four hours. Comparisons of the heating effect with the radio-balance, and of the γ-ray activity with the electroscope were made daily between the radium and the emanation. It was found that the heating effect of the radium could be represented by a nearly constant quantity 70 microwatts plus the heating effect of a quantity of emanation equivalent to its γ-ray activity. The value finally attained by the radium was 255·4 microwatts, of which 70 microwatts, or 27·4 per cent., was apparently due to the radium alone, and the remainder 72·6 per cent. to the emanation in radio-active equilibrium with it. This is probably not quite accurate, because the radium, being an old sample, should have contained an appreciable proportion of polonium, which would affect its evolution of heat to a somewhat uncertain extent. Moreover, the rate of recovery observed did not accurately coincide with a period of 3·86 days, but was appreciably slower, and the radium was still 2 per cent. below its final value at the end of the first month.

The sample of radium enclosed in the small glass bulb, and fitted with a capillary tube handle, afforded a very convenient means of testing various types of cup radio-balance, and investigating the conditions of sensitiveness. All the cup balances made were tested in this way at various times. Although they differed greatly in sensitiveness and in details of construction, and some were quicker and more accurate than others, they all agreed within 0·5 per cent. in the values found for the radium on any given date. With the more accurate and sensitive balances, the values were consistent to 1 in 1,000, which is about the limit of accuracy in the determination of the thermoelectric power of the couples employed. The following example of observations taken with one of the most sensitive radio-balances, will serve to illustrate the order of accuracy attainable.

In taking readings, the radium bulb was placed in one cup (after it had been reduced to the temperature of the radio-balance by being allowed to remain for five minutes in a special cup provided for the purpose inside the copper cylinder), and a precisely similar glass dummy bulb similarly treated, was

Radio-Balance E. *7-ohm Galvanometer.* *September 20th,* 1910. *Radium* 1·80 *mgm.*

—	Radium deflection. Ra and dummy bulb.		Balance method. Ra and dummy bulb.		Current deflection. Two dummy bulbs.		
Ra in cup …	(1)	(2)	(1)	(2)	(2)	(1)	(2)
Temp. C. .—	15·20°	15·22°	15·26°	15·28°	15·30°	15·33°	15·36°
Current m.a..	—	—	−8·035	+8·032	−8·030	+8·029	−8·028
Deflection …	+200·2	−202·7	+10·5	−13·3	−190·3	+188·8	−190·3
Results ……	Ra = 402·9 sc. = 255·4mw.		Ra = 240·3mw. + 23·8sc. = 255·4mw.		240·3mw. = 379·1sc. 1·5775 sc./mw.		

placed in the other cup, so as to balance the external conditions. Owing to the thermal capacity of the glass bulbs and their low thermal conductivity, the deflection took three or four minutes to become steady, and readings were taken at five-minute intervals. The radium and dummy bulbs were interchanged, and the opposite deflection taken. In the balance method, the current was reversed simultaneously with the interchange of the bulbs. In observing the deflection with the current alone, the radium was replaced by a dummy bulb, in order to compensate the loss of heat by conduction along the capillary glass handle. Under these conditions the deflection method always gave results agreeing very closely with the balance method, unless the temperature was varying rapidly, in which case the balance method was appreciably more accurate.

14. *Absolute Value of the Heat Emission of Radium.*

Although the relative values of the heat emission of samples of radio-active materials contained in similar bulbs may be compared with considerable accuracy by means of the radio-balance, the absolute values cannot be determined with the same degree of precision. The absolute values are dependent on comparisons made with the electroscope, the accuracy of which depends on the behaviour of the electroscope, and on the purity and constancy of the standards of comparison. The absolute value of the heat emission as measured by the radio-balance also requires correction for the fact that the radium bromide is enclosed in a glass bulb of low thermal conductivity, and is not in metallic connection with the copper cup like the peltier couples. More heat is lost by conduction along the capillary glass stem from the radium than from the copper cup or the peltier couples. The latter source of error might, no doubt, have been avoided by enclosing the radium completely in the cup, but this would have increased the difficulties of manipulation so greatly that the accuracy of the relative comparisons would have been seriously diminished. It was therefore considered preferable to determine this source of error and apply a correction. This was done by enclosing a standard thermo-couple with its two junctions in oil in a pair of glass bulbs with capillary tubes similar to the radium and dummy bulbs, and observing the current through the standard couple under these conditions required to balance a current through the peltier couples equivalent to the radium. In this way the various resistances to the flow of heat, and the losses by conduction along the capillary glass stems of the various bulbs, could be determined exactly as in an analogous electrical problem. The losses by conduction along the leading wires of the various couples and resistance coils employed in testing and calibrating the radio-balances were similarly determined. A similar source of error affects all comparisons in which it is attempted to compensate the heat emission of radium by heat generated in a resistance coil or couple with connecting wires. Angström employed for this purpose a resistance coil of only 0·365 ohm with a current of 140 milliamperes, and does not appear to have applied any correction for the heat generated in the leading wires, or for the loss of heat by conduction, neither of which were compensated. These effects must have been relatively large, and could only compensate each other by accident. With a 10-ohm coil and a current of 3 milliamperes, the errors were found to be quite appreciable with the radio-balance, and were in all cases carefully measured or compensated.

The loss of heat by conduction from the copper cups along the capillary glass stems of the dummy or empty bulbs, was easily measured by taking observations of the deflection due to a current through the peltier couples with and without the bulbs in place. The conductance could be expressed in microwatts per degree of temperature, or more conveniently in microwatts per scale division deflection. The resistance to the escape of heat, or the reciprocal of the conductance, is similarly expressed in scale divisions per microwatt. The resistance to the escape of heat from the copper cup by itself is denoted by Q. The resistance to the escape from the copper cup along the capillary glass stem is the sum of the resistance R between the copper cup and the glass bulb and the resistance S between the glass stem and the enclosing walls of the cylinder. The resistance to the escape of heat from the bulb itself along the glass stem is S alone, but the resistance through the copper cup is Q+R. In the first case, Q is in parallel with R+S, in the second Q+R is in parallel with S. The supply of heat to the inside of the glass bulb required to produce the same rise of temperature in the pile (or the same heat flow through Q) as heat directly communicated to the cup by the peltier couples, must be greater than the latter in the proportion of R+S to S. The correction was slightly different for the various bulbs, but lay between the limits 6 and 7 per cent. For the radium bulb it was 6·8 per cent. The heat supply 255·4 microwatts to the peltier couples required to balance the radium, must be increased in this proportion, giving 272·7 microwatts as the actual heat emission of the radium bulb.

The quantity of radium transferred from the original tube to the small bulb was determined by estimating the quantity remaining in the tube by means of the electroscope. The remainder was found to be 9·5 per cent. of the original 1·99 milligrams. Assuming none of the radium to have been lost in the transfer, this would make the contents of the bulb equivalent to 1·80 milligrams of radium, or 3·08 of $RaBr_2$. The rate of heat emission finally attained would therefore be 0·1515 watt per gramme of radium, or 130·5 gramme calories per hour. This is a minimum estimate, but includes most of the energy of the β-rays. The most uncertain factors in the calculation appear to be the exact radium content of the sample, and the effect of age on the heat emission. Generally speaking, all the phenomena observed agreed very fairly with the account given by Prof. Rutherford in his investigation of the heat emission of radium and its emanation (*Phil. Mag.*, February, 1904), but some facts were observed which appear to require further investigation, and it is possible that more accurate comparisons of the radio-activity and the heat emission may throw further light on some of the problems of the disintegration theory.

ABSTRACT.

In this apparatus, which was first constructed in 1905, heat supplied by radiation is directly compensated by the peltier absorption of heat in a thermo-junction through which a measured electric current is passed. In the simplest form of the instrument, radiation admitted through a measured aperture 2 mm. diameter falls on a small copper disc 3 mm. diameter by 0·5 mm. thick, to which two thermo-junctions

are attached, forming a peltier cross. One couple is connected to a sensitive galvanometer for indicating changes of temperature. The other is connected to a battery and rheostat in series with a milliammeter or potentiometer for measuring the current required to reduce the deflection of the galvanometer to zero. If A is the area of the aperture in square centimetres, H the intensity of the radiation in watts per square centimetre, a the absorption coefficient of the surface of the disc, P the peltier coefficient in volts, C the balancing current in amperes, and R the effective resistance of the couple, the equation giving the value of the radiation in absolute measure is

$$aAH = PC - C^2R.$$

The absolute value of P is the product of the absolute temperature by the thermoelectric power. The value of R, in the small correction term for the joule effect, is readily determined by observing the neutral current, $C_0 = P/R$, for which the joule effect balances the peltier effect. In practice two similar discs with similar connections are mounted side by side in a thick copper box, and are balanced against each other in order to avoid changes of zero due to exposure to sunshine, or rapid variations of temperature.

The advantages of the disc radio-balance are that it is very simple to construct and easy to reproduce without material variation in the reduction constants. It is very suitable for measurements of solar radiation, or strong sources, but is insufficiently sensitive for weak sources; and the absorption coefficient a must be determined by comparison with a standard.

In the cup radio-balance the radiation is received in a copper cup 3 mm. diameter by 10 mm. deep, so that the absorption coefficient is practically equal to unity. Greater sensitiveness is secured by employing a pile of several couples, insulated from the cup, in place of the single balancing couple. External disturbances are eliminated by employing a pair of cups, similarly mounted but oppositely connected, enclosed in a thick copper cylinder. The joule effect, represented by the C^2R term in the equation, is automatically eliminated by passing the same current in series through the opposing peltier junctions soldered to the bottoms of the cups. The cup exposed to radiation is cooled, and the cup screened from radiation is heated by the peltier effect, while both are equally heated by the joule effect. A complete observation involves reversing the current and switching over the radiation screen, in order to eliminate any difference of sensitiveness of the two piles. By observing the neutral current, each cup can be used separately, as with the disc balance; but the disc balance cannot be used with the peltier couples connected in opposition, unless the balancing couples are insulated from the discs. The cup radio-balance is sensitive to less than a tenth of a microwatt, and is very suitable for measuring the heat evolved by small quantities of radio-active substances. It was applied to radium at Prof. Strutt's suggestion, and Prof. Rutherford very kindly supplied samples of emanation, and determined the value of the radium sample employed by comparison with his own standards. The second sample of emanation had only just come to hand, and the absolute values had not been finally reduced at the time the paper was read; but it appeared from the preliminary reductions that the heat evolution of radium in terms of Prof. Rutherford's standards was somewhat greater than that given by previous observers.

The instruments exhibited at the meeting included (1) a disc radio-balance equatorially mounted for solar radiation; (2) a laboratory pattern, with water or steam jackets for determining the temperature coefficients and for investigating the theory of the instrument; (3) a cup radio-balance, with which the heat production of radium and its emanation were demonstrated. The latter intrument is capable of measuring the heat evolved from 1 milligram of radium to about one part in 1,000.

DISCUSSION.

Dr. J. A. Harker expressed his interest in the paper, especially in the details of the construction of the various instruments exhibited.

Mr. A. Campbell asked what were the materials of the wires used for the thermo-couples.

The President, in reply to Mr. Campbell, said he had used iron-constantan couples.

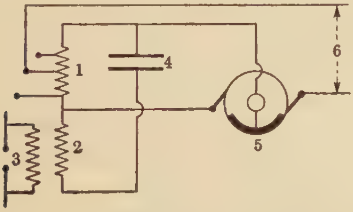
II *Demonstration of a New Method for producing High-tension Discharges. By* Prof. ERNEST WILSON *and* Mr. W. H. WILSON.

OCTOBER 28, 1910.

[ABSTRACT.]

ACCORDING to this method, energy is taken from an alternating or continuous-current source and stored in a magnetic field by an inductance; it is then permitted to surge into a condenser which forms with the inductance a *low* frequency oscillating circuit. When the energy is stored in the condenser the latter is mechanically bridged across the primary winding of a spark-coil, with which it forms a *high* frequency oscillatory circuit. The energy is then transmitted by the secondary winding of the spark-coil to the work circuit in a well-known manner.

The method, therefore, involves the use of an inductance, a condenser, a spark-coil and a special contact-making device.



1=Inductance.
2=Primary winding.
3=Secondary winding.
4=Condenser.
5=Contact maker.
6=Supply mains.

In one form the contact maker consists of a cylinder of insulating material driven by a small motor, the cylinder carrying a metal segment, and a complete ring of metal attached to the segment. Three brushes (say, A, B, C) bear upon the cylinder. A makes contact with the segment once in a revolution, B is continuously in contact with the ring, and C makes contact with the segment once in a revolution, and is mounted on an adjustable arm. According to one method of connection, the coil of the inductance is connected in series with the primary winding of the spark-coil and forms one circuit. In parallel with this circuit is placed the condenser. One pole of the supply source is connected to brush A on the contact maker. The other pole of supply is connected to a tapping point on the inductance suitable for the voltage of supply, the total number of turns on the inductance depending upon the voltage at which it is desired to charge the condenser. The junction of the inductance and the condenser is connected to brush B on the contact maker, and is, therefore, continuously in contact with the metallic segment. The junction of the inductance and the primary winding of the spark-coil is connected to brush C, which is capable of adjustment.

At the epoch of "make," the brushes A and B are connected by the segment, and the system is switched on to the supply source. The current rises in the winding of the inductance and builds up the magnetic field. The condenser also becomes charged, and its charging current passes through the primary winding of the spark-coil. At the period of "break," brush A severs contact with the segment and is insulated. The energy stored in the condenser is added to that in the inductance, and the condenser begins to be charged in the reverse direction. Ultimately the whole energy in the system is stored in the condenser. Brush C is so adjusted that, at this moment, it begins to touch the segments of the contact maker, and thereby short-circuits the inductance. The condenser then discharges with great rapidity through the primary winding of the spark-coil, and the energy oscillates to and fro. As is well known, the E.M.F. induced in the secondary winding may be of an oscillatory or unidirectional character, according to the nature of the work circuit. As only the current in the condenser passes through the primary winding of the spark-coil at "make," the inverse E.M.F. induced in the secondary winding is so small as to be negligible, but if desired it can be entirely eliminated by a modification in the connections.

Briefly, the following are some of the advantages gained :—

1. For X-ray work the inverse E.M.F. at "make" is eliminated, thereby leading to increased life of the tube and to a more sharply defined radiograph.

2. Only a small magnetising current is required as the inductance has a nearly closed magnetic circuit. This gives rise to a very small C^2R loss, and consequently higher efficiency.

3. On account of the long periodic time of the system between the periods of "break" and "short-circuit," the voltage across the contact of the interrupter at "break" does not rise to a high value, or rises so slowly that the contacts are well separated before it is developed. Hence immunity from sparking.

4. The method lends itself to few secondary turns, and this keeps down the time constant. It also makes the coil lighter, cheaper and more compact.

5. The method lends itself to low secondary resistance—a point of great importance in connection with radio-telegraphy.

6. The iron of the spark-coil can be kept small in amount, and special attention can be paid to its lamination and insulation, as it may have to be operated at frequencies of three or four thousand per second.

7. The elimination of sparking increases efficiency, and on board ship, where coal gas is not conveniently obtainable, this is an advantage.

8. The apparatus can be worked from direct-current or alternating-current systems at usual voltages, or from a portable battery of a few storage cells.

9. The oscillatory current at "break" does not pass through the battery, and hence does not assist in its discharge.

10. The iron in the magnetic circuit of the external inductance has only to operate at low frequency, and hence it has not to be finely laminated.

11. When used on alternating-current systems rectification, if desired, is easily effected by employing two short-circuiting brushes, one for each half period, and allowing the second brush to short-circuit at the moment when the condenser is fully charged after allowing a second complete surge of the energy between the condenser and the inductance of the spark-coil.

12. The apparatus is light, efficient and cheap, and is suitable for radio-telegraphy, X-ray, medical and other work in which high-tension electricity is employed.

The following are a few particulars of the apparatus exhibited at the meeting :—

Spark length of secondary winding	10 in.
Secondary turns	16,000 to 20,000
Primary turns	78
Secondary resistance (aluminium)	2,900 ohms
Primary resistance	0·056 ohm
Weight of spark coil and condenser	10 lb.
Weight of inductance and interrupter	20 lb.

DISCUSSION.

Mr. A. CAMPBELL asked if the adjustment of the commutator was made experimentally, and if the instrument could produce high-frequency oscillations suitable for wireless telegraphy.

Mr. W. **Duddell** asked if the discharge through the X-ray bulb in the experiment shown at the meeting was unidirectional.

Prof. J. A. **Fleming** asked if the coil could be used for medical work. He also asked if the frequency of the discharge could be made high enough for wireless telegraphy, and commented on the small amount of sparking at the commutator.

Dr. R. S. **Willows** suggested that the nature of the discharge in the secondary could be easily determined by using a vacuum tube rectifier.

Mr. **Addenbrooke** asked how much energy could be obtained from the secondary of the coil exhibited.

Prof. **Wilson**, replying, said the adjustment was performed experimentally. Using an air coil for primary, enormous frequencies could be obtained in the secondary. The efficiency of the instrument was high compared with that of ordinary coils. The E.M.F. induced in the secondary on open circuit was oscillatory, but when loaded the damping could be such as to cause the E.M.F. to be unidirectional. In the experiment shown with an X-ray bulb the discharge was nearly unidirectional.

III. *The Behaviour of Steel under Combined Static Stress and Shock.* *By* F. Rogers.

Received August 30, 1910. Read October 28, 1910.

The behaviour of materials, particularly steel, under rapidly applied stress—equally vaguely described as "shock"—is probably at once the most difficult and the most important branch of elasticity which yet remains to be fully investigated.

Investigators who have approached the problem from the point of view of determining what steels possess in the highest degree the useful property of offering a great resistance to rupture under a blow, or detecting a lack of this property, seem generally to have had to content themselves by choosing a convenient "velocity of tup" or "speed of shock," which has been determined largely by the limitations of the apparatus and type of test sample selected. No information as to the stresses set up by the blow in tests carried out in these machines is obtainable; and, of course, up to a certain point, such information is not necessary.

Under this category we have the machines of the pendulum class, such as those of Charpy and Izod, the rotating toothed disc of Guillery and the falling weight of Frémont, in which notched bending test-pieces are used, so that the determination of the stresses due to the blow is impossible.

In the falling-weight machines of the types designed by Hatt, and by Stanton and Bairstow, the great advantage of having direct, instead of bending, stresses is achieved, but in the present forms the determination of stresses would be attended with uncertainty, and appears not to have been attempted.

On the other hand, considerable attention has been devoted to the mathematical study of the propagation of stress in a perfectly elastic body upon which another impinges, and in the past few years the mathematical theory, apparently first approached by Newton, has been experimentally verified by J. and B. Hopkinson,* Sears † and others. The mathematical theory is based upon conditions and assumptions which have as result that each section successively of a rod which receives a longitudinal blow at one end is *instantaneously* jerked to a maximum stress which gradually dies away. Graphically, the wave of stress, with an absolutely steep front, advances along the rod with the velocity of sound in the material of the rod.

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* Royal Soc., Proceedings, Series A, 1905, pp. 498-506.

† Cambridge Phil. Soc., Trans. 21, No. 2, pp. 49-105, August 25, 1908.

Between this extreme, and infinitely slow ("static") loading, all gradations of the rate of increase of stress do occur—*i.e.*, it is possible to have any slope of wave front; this slope is the rate of increase of stress. The definite time lag in the growth of the stress to a maximum value is the same for all rates of loading in the same material, and with the same kind of stress; in other words, the rate of translation of the wave of stress is independent of its shape. Thus, when a longitudinal pull, push or blow is applied at one end of a bar of steel about 3½ miles long it is always one second later when the same forces exactly are felt at a point towards the other end of the bar. The decay of these forces and their subsequent complication by reflection need not be considered here.

In addition to affording some confirmation of the mathematical theory of the propagation of stress, B. Hopkinson's experimental investigation just quoted, in which a weight was dropped upon a stop attached to the lower end of a long hanging wire, showed that if the stress exceeded the elastic limit for 1/1000 second or less it might exceed even the so-called static ultimate stress without the occurrence of rupture or permanent deformation of the wire. In tests by this method rupture was unaccompanied by any considerable permanent extension of the wire.

The fundamental physical importance of the rate of increase of stress at a given point or section in a piece of material has, however, received scant recognition. It is precisely this variable, *the time rate of increase of stress ds/dt*, which it appears to me makes all the difference when we consider the behaviour of a metal which is capable of elongation under "static" loading. This seems to have been, somewhat indistinctly perhaps, the quest of those workers who from time to time have made tests of steel at various "speeds of loading" within the range obtainable by means of ordinary tensile testing machines, and who have generally concluded that within the usual speeds the variation of properties is only slight. Again, shock tests have been made with various "speeds of tup" (sometimes called "speed of shock" or "velocity of shock"); or keeping the striking velocity and energy of the tup the same for each test, the sharpness of the notch has been varied.

Despite the occasionally conflicting nature of the evidence it suggests that, in a general sense, as the *rate of increase of* *stress* is increased the nature of the breakdown is essentially altered. The deformation sustained by the material becomes less, the distribution of the deformation is altered, and usually the energy absorbed in rupturing the piece becomes less.* That there is an essential difference in the intimate nature of the deformation is shown by the microscopical investigations of Osmond, and of Stanton and Bairstow, and others. There is doubtless a progressive change as the rate of increase of stress becomes greater. At low rates of increase of stress the deformation occurs principally by crystalline cleavages, recognised as slip-bands, and at high rates Neumann lamellæ are formed. This is illustrated to a certain extent by the difference often noticed between the fracture of a piece which has " broken short under a blow " and an ordinary steady tensile or bending test fracture, especially, *e.g.*, in the case of common bar iron.

The exact determination of the rate of increase of stress will admittedly present much difficulty whenever it amounts to what is commonly designated " shock." In a few simple cases, it is true, the rate of growth of stress within the elastic limit can be directly calculated ; but this is not of much help, as our final appeal will probably be rather to measurements of the energy required to produce fracture of a specimen than to measurements of strains. In the simplest method of determining the energy required to rupture a specimen—viz., bending tests of notched pieces—the calculation of stresses becomes extremely difficult, owing to the effects upon their distribution due to (*a*) the fact that the specimen is a beam under central load, (*b*) the fact that the specimen is notched, and (*c*) the rate at which the load is applied ; and owing to the difficulties of recording the relation of time and the pressure between the tup and the test-piece.

Nevertheless, several more or less indirect methods can be devised, and it is now proposed to record the results of some preliminary experiments made in one such way. It is remarkable that this indirect method simultaneously yields additional intrinsically valuable information.

In my experiments, specimens of steel were tested by shock whilst supporting a steady load. Keeping other factors constant, a high rate of increase of stress is thus attained when the blow is applied. The question whether the behaviour of steel under shock depends (apart from the reason of any dependence) upon the value of any initial static stress in the steel appeared in itself to be sufficiently important to warrant investigation, for static stress and shock act frequently in combination in structures.

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* In order to avoid cumbrous circumlocution, a sufficient use of " ceteris paribus " is taken for granted.

The tests were made by means of an apparatus whose cost was borne by the Government Grant Committee of the Royal Society. Its peculiarity of design is in part due to the fact that it was contemplated to use it for several experimental methods of approaching the general problem of the effect of rate of increase of stress. It is a shock testing machine of the pendulum type. A general view is given in Fig. 1. Not only, however, is the hammer (weight 60 lb.) suspended, but the anvil (weight 260 lb.) also forms another pendulum, the two having a common axis of suspension and common radius of gyration (3·80 ft.). The specimen rests on two supports at the centre of percussion of the anvil, and the striking point of the hammer is at the centre of percussion of the hammer. The movements of the pendulums are indicated by two pointers carried over a dial. From the movements the energies are calculated.

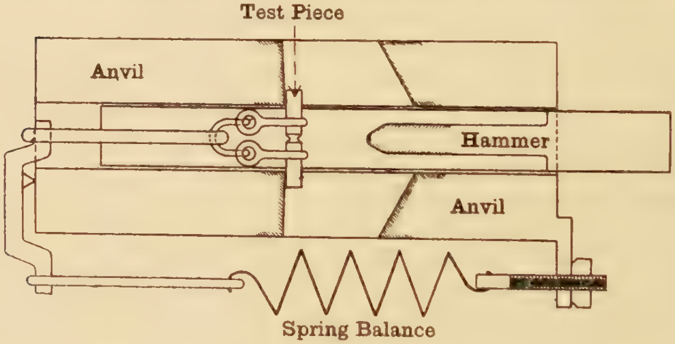

FIG. 3.—DIAGRAMMATIC SECTION.

The maximum energy of blow available is 440 ft.-lb.*

As represented in the photograph, Fig. 2, and diagrammatically in Fig. 3, the static bending load up to 800 lb. is applied,

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* The only other shock testing machines of which I have been able to find any record in which both hammer and anvil are free to move are (1) that of F. Kick, which was used for shock compression tests only ("Das Gesetz der proportionalen Widerstände," 1885; Leipzig, Arthur Felix), and (2) one used at the National Physical Laboratory, but which I believe is not at present in use (Inst. Mech. Engineers, Proc., A., pp. 857-959, October-December, 1905).

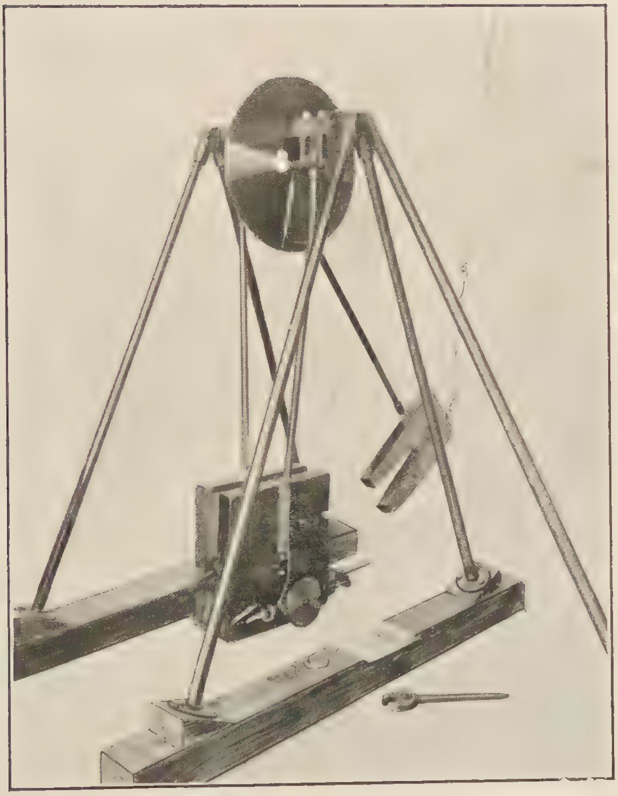

Fig. 1.

To face p. 42.

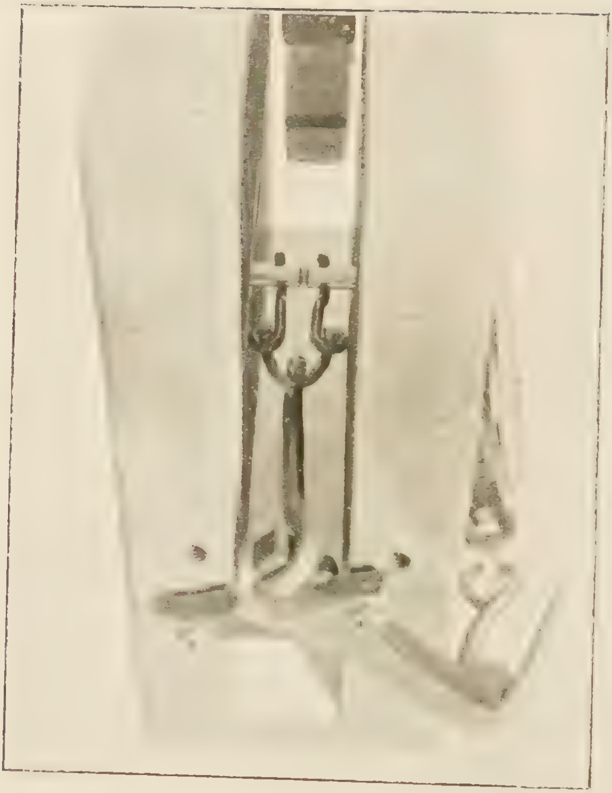

Fig. 2.

in the same direction as the blow is to be applied, at two points on the specimen equi-distant from its centre by means of a light combination bearing upon the anvil and consisting of a nut, screw, lever, system of links and spring balance. The links are arranged to leave the centre of the specimen clear to receive the nose of the hammer. When the hammer strikes the specimen the static pull is released naturally in consequence. A spring was selected for the static loading in preference to dead weights in order to avoid inertia effects and dynamic losses.

A rough calculation shows that, with the gear and striking velocities adopted, when the test piece is struck the static pull is released in a negligibly short distance, and is not caught up again.

RESULTS OF TESTS.

Table I. gives the results of a series of experiments upon test pieces of a mild steel of the following percentage composition :—

Steel A.

C.	Mn.	Si.	P.	S.
0·16	0·45	0·008	0·019	0·027

Bars ½ in. diameter, reeled.

TABLE I.

Diameter of test pieces	0·500 in.
Diameter at bottom of groove	0·240 in.
Span	2·18 in.
Striking energy of hammer	230 ft.-lb.
Striking velocity of hammer	15·6 ft. per second

The sides of the groove were inclined at 60 deg. to one another and joined by a curve of about $\frac{1}{50}$in. radius. All were formed by the same tool.

" Calculated extreme static stress." Tons per square inch. (*See* explanation below.)	Energy absorbed in rupturing test piece by shock, corrected for energy transmitted to anvil. Foot-pounds.	Energy transmitted to anvil. Foot-pounds.	Remarks.
0	35·27	0·20	All from one bar
21·0	32·75	0·25	
40·5	31·23	0·28	
56·7	21·96	0·09	
64·8	20·79	0·07	
72·9	24·73	0·10	From another bar of the same lot of steel
80·6	24·71	0·12	

What has been called, for want of a better term, the "calculated extreme static stress" in the first column, is the greatest stress which would be produced in a beam of uniform circular cross-section having the same diameter as the test piece has at the bottom of the groove and with the same static loading, assuming the material of the beam to be perfectly elastic up to such a stress. The value is thus directly proportional to the load, but not necessarily to the actual stress existing in the piece, especially at the higher loads, when it does not lend itself to calculation, but is certainly less than the value given.

These results are plotted in Fig. 4, curve A.

They indicate that as the static stress is increased there is a very considerable falling off in the energy absorbed in rupturing the steel by shock, amounting at the highest loads to about 30 per cent.

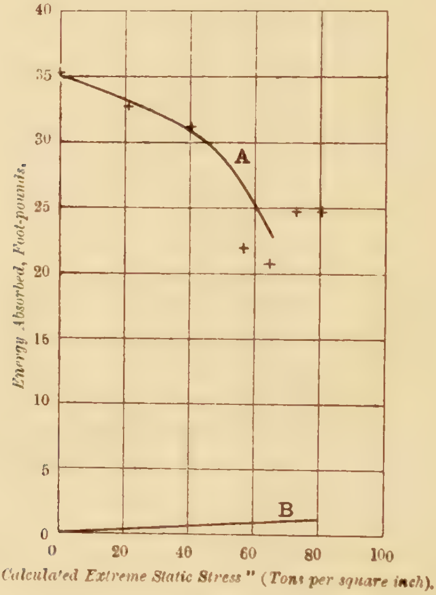

FIG. 4.

Curve A.—Energy absorbed in rupturing test piece by shock.
Curve B.—Work done on test piece by the static load.

It is important to notice that this falling off is in all cases much greater than the work which the "static" load does upon the test piece. An approximate representation of the work done by the static load is given by curve B, which is calculated from observed deflections. With the highest static load this is only about 5 per cent. of the energy required to rupture the piece by shock alone under the conditions adopted.

A few further results will now be given for tests in which one or more of the conditions was varied from those adopted in the above series. In each of these experiments two identical test-pieces were tested with a high static stress and with little or no static stress respectively, as recorded below.

Table II.

All test pieces were of 0·500 in. diameter and 2·18 in. span. The groove was of the same form for all as described in Table I. The static loading arrangement was the same for all, and was in place upon the specimen whether any static load was applied or not.

Steel A.—Composition as above.

Diameter at bottom of groove .... 0·224 in.
Striking energy of hammer .... 230 ft.-lb.
Striking velocity of hammer .... 15·6 ft. per second

"Calculated extreme static stress." Tons per square inch. (*See* explanation above.)	Energy absorbed in rupturing test piece by shock, corrected for energy transmitted to anvil. Foot-pounds.	Energy transmitted to anvil. Foot-pounds.	Remarks.
0	26·94	0·08	...
71·6	19·41	0·08	Very slight permanent set perceptible under the static load

The observation in the last column indicates that the elastic limit had been reached at a considerably lower load than was used.

Steel B.—Composition per cent.

C.		Mn.		Si.		P.		S.
0·45	......	0·55	......	0·093	......	0·017	......	0·030

Bars ½ in. diameter, reeled.

Diameter at bottom of groove. Inches.	"Calculated extreme static stress." Tons per sq. in. (*See* explanation p. 43.)	Striking energy of hammer. Foot-pounds.	Energy absorbed in rupturing test piece by shock corrected for energy transmitted to anvil. Foot-pounds.	Energy transmitted to anvil. Foot-pounds.	Remarks.
0·230	4·5	230	16·99	0·04	...
	92·0		9·81	0·02	...
0·170	0	77	7·65	0·03	A similar test piece gave an appreciable permanent set at 95·6 tons per sq. in. "calculated extreme static stress"
	95·6		5·48	0·02	

Steel C.

Carbon about 0·25 per cent.

Bars $\frac{5}{8}$ in. diameter, reeled.

Diameter at bottom of groove .... 0·200 in.
Striking energy of hammer .... 77 ft.-lb.
Striking velocity of hammer .... 5·2 ft. per second

"Calculated extreme static stress." Tons per square inch. (*See* explanation above.)	Energy absorbed in rupturing test piece by shock, corrected for energy transmitted to anvil. Foot-pounds.	Energy transmitted to anvil. Foot-pounds.	Remarks.
0	9·52	0·03	A similar test piece gave an appreciable permanent set at 92·2 tons per square inch "calculated extreme static stress"
92·2	5·48	0·02	

These results point to the same conclusion as those in Table I.—viz., that much less energy is absorbed in rupturing specimens by shock if they be under static stress than if they be initially unstressed.

It is interesting to note incidentally that the energy transmitted to the anvil is almost negligibly small in all the tests recorded here.

CONCLUSIONS.

The important conclusion that *steel is substantially less resistant to shock whilst it is under static stress* appears to be definitely established in a preliminary fashion by these experiments. It is noteworthy that the correction for the work done in applying the static stress is small; that is to say, that *the energy absorbed in breaking a sample of steel is greater when entirely applied as shock than when applied partly as shock and partly statically.*

The explanation of this difference is chiefly, I think, the difference in the rate of increase of stress at the higher stresses in the two cases. The actual values of the highest stresses which occur are not necessarily identical. In the light of B. Hopkinson's result (already quoted, *see* p. 40) and the present experiments, I would infer that, for a given steel, the higher the rate of increase of stress the higher is the stress reached before rupture occurs. At the same time, as already mentioned (*see* p. 41), the total deformation of the piece, speaking generally, becomes less, and the intimate nature of the breakdown may also be expected to show a corresponding variation.

It is possible that there may be an additional cause of the loss of toughness of steel occasioned whilst it is under static stress, which depends directly upon the nature of the intermolecular forces. In the present state of knowledge, however, such an explanation would be highly conjectural, and is therefore passed over.

At the higher static stresses employed, test pieces were perceptibly strained beyond their elastic limits, and this may also help to account for a part of the diminution in the resistance to shock.

The author is inclined to attach some, but relatively little, importance to this factor. It must not be inferred from this, however, that I am prepared to admit that a piece of steel slightly overstrained statically would *upon removal of the static load* be any more brittle under shock than if it had not been so strained. The contrary is indeed sometimes the case.

The investigation has not been carried far enough to show the precise relation between the energy absorbed in rupturing and the rate of increase of stress. Experience would lead one to expect that the relation would be different for different steels—that the energy absorbed decreases comparatively rapidly as the rate of increase of stress rises in the case of steels alleged to have given good tensile test results but poor resistance to shock.

Attention has been drawn by the writer * to the possibility that considerable internal stresses arise in steels during cooling, due to the difference between the coefficients of expansion of ferrite and pearlite. As these stresses are to be expected to depend upon many factors, as, for example, the size and shape of the individual constituent grains, they will doubtless vary considerably from point to point throughout a bar of steel, and would probably not be eliminated by annealing. In the light of the present results, this affords a new partial explanation of the variations which investigators have at times experienced in the results of shock tests of samples cut from the same piece of steel, for the resistance of steel to shock is very sensibly dependent upon the value of any static stress which exists in it.

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* Iron and Steel Institute Journal, 1905, 1, p. 489.

The writer thanks Messrs. Edgar Allen & Co. and Messrs. Thos. Andrews & Co. for some of the samples of steels which he has employed in this investigation.

ABSTRACT.

Attention is drawn to the importance of the time rate of increase of stress, *ds/dt*, in the behaviour of materials under stress. The exact determination of this rate must usually present much difficulty, but the in direct experimental method adopted consisted in submitting specimens of steel to shock whilst under static loading. The machine used for this purpose was exhibited and described at the meeting, and contained some novel features. The important conclusion that steel is substantially less resistant to shock whilst it is under static stress appears to be definitely established by these experiments. In some cases the effect of a large static stress was to diminish the resistance to shock by as much as 30 per cent.

It is noteworthy that the correction for the work done upon the sample in applying the static load is relatively small. Thus, the energy absorbed in breaking a sample of steel is greater when entirely applied as shock than when applied partly as shock and partly statically. This difference is considered to be due chiefly to the difference in the rate of increase of stress at the higher stresses in the two cases. The actual values of the highest stresses are not necessarily identical, but probably the higher the rate of increase of stress the higher is the greatest stress reached before rupture occurs; whilst simultaneously deformation is diminished, and the intimate nature of the breakdown suffers a corresponding variation. An explanation depending directly upon the nature of intermolecular forces is so conjectural in the present state of knowledge that it is not pressed. At the higher static loads employed some portions of the test pieces were stressed beyond their elastic limits, and this may also help to account for a part of the diminution in resistance to shock—a factor to which the author is, however, inclined to attach comparatively little weight, because a piece of steel which has been overstrained statically is not necessarily thereby made more brittle. The importance of the effect of internal stresses in steel upon its resistance to shock is emphasised in view of the present results.

DISCUSSION.

Mr. R. S. WHIPPLE asked if the same results were obtained with high and low carbon steels, and whether the results were affected by annealing.

Mr. ETCHELLS asked what the author meant by "calculated extreme static stress," and also how the stress was applied. It was difficult to calculate the stress in the author's experiments.

Prof. SMITH said the paper opened up a field for research, and then described some experiments which he had been making recently on the time rate of change of strain.

The AUTHOR, in reply to Mr. Whipple, said he had obtained the same results from steels with carbon varying from 0·16 to 0·45 per cent. The actual apparatus was exhibited and the manner in which the stress was applied was shown.

IV. *On the supposed propagation of "Equatorial" Magnetic Disturbances with velocities of the order of* 100 *miles per second.* *By* C. Chree, *Sc.D., LL.D., F.R.S.*

Received October 14, 1910. Read November 11, 1910.

In a recent number of "Terrestrial Magnetism,"[*] the question of the simultaneity of magnetic disturbances recorded at different stations has been discussed by Dr. L. A. Bauer[†] and Mr. R. L. Faris.[‡] A good many magnetic storms have so-called "sudden commencements," an outstandingly rapid change of force occurring, often without any previous indication of disturbance. As regards these "sudden" changes, three things are conceivable: They may be absolutely simultaneous at different stations; there may be a very small difference of time, corresponding to the rate of propagation of electromagnetic waves; or, finally, there may be, as Dr. Bauer concludes, longer intervals, amounting to several minutes, for stations remote from one another. Under existing conditions of registration one cannot decide between the first two possibilities. As between these two and the third a decision should not be impossible. The best, if not the only, way of settling the question is by reference to actual records, but there are a variety of general considerations which should be borne in mind in any such comparison.

If a magnetic storm is due to any form of electric current, whether overhead or underground, the commencing disturbing force is unlikely—except accidentally and on rare occasions—to be wholly in or wholly perpendicular to the local magnetic meridian, or wholly along or wholly perpendicular to the local vertical. It would, for instance, be an extraordinary kind of external disturbing force which commenced by changing the declination and nothing else all over the world. Thus it is probable that, except on rare occasions, sudden commencements represent forces having components in the directions alike of D, H and V (Declination, Horizontal Force and Vertical Force). This is quite consistent with the disturbance being visible earlier in the trace of one element than in those of the others. These "sudden" commencements represent, at least as a rule, a force gradually increasing from zero, and the exact time at which the movement becomes apparent in a magnetogram depends on the sensitiveness of the particular instrument—in instruments of the same type mainly on the openness of ordinate scale—and on how nearly the disturbing force is perpendicular to the magnet's axis.

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[*] Vol. XV., No. 2, 1910.
[†] *L.c.*, p. 107.
[‡] *L.c.*, p. 93.

Mr. Faris' figures from the U.S. Coast and Geodetic Survey stations show a decided tendency for commencing movements to be earliest visible in the H trace and latest in the V (Mr. Faris' Z). This probably means only that in most of the cases considered the disturbing force had a larger component in than perpendicular to the magnetic meridian—a phenomenon * already noted at a variety of stations—or that the change of force answering to 1 mm. of ordinate was less for the H than for the D trace. The fact that the V commencement tended to be the latest arose probably in part from the fact that the commencing vertical force is usually considerably less * than that in the horizontal plane, but not improbably in part from a slight frictional opposition to movement in the vertical force magnets.

If circumstances were absolutely alike at all stations, then Mr. Faris' plan of deriving the interval between commencements at different stations by comparing the D, H and V records separately would be more satisfactory than it actually is. When, however, we compare two distant stations—especially when their latitudes are so different as those of Sitka and Porto Rico, two of the Coast and Geodetic Survey stations—the disturbing forces are likely to differ considerably in amplitude and in orientation relative to the local magnetic meridian and vertical. If at both places the H component predominates and is large, and the two H instruments are equally sensitive, then a comparison of the commencing times shown by the two H traces may be the best way of measuring the apparent difference in time; but it obviously may not be so if the H component at one station is much smaller than at the other. This probably partly explains the large differences between the results which Mr. Faris obtains in the case of the same storm from the D, H and V curves treated separately. For instance, the time for propagation of a disturbance completely round the earth which he derives from his example No. 5 varies from 0·5 minute when V curves are considered to 11·6 minutes when H curves are considered.

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* National Antarctic Expedition, 1901–1904. Magnetic Observations. Chap. IX., especially p. 181.

Assuming the disturbing force finite and continuous, then, however large it may be, some time must elapse before the change in the curve ordinate is measurable. Thus all traces naturally post-date the commencement, and, apart from instrumental error, if we take the earliest time shown by *any* trace, whether D, H or V, we cannot ante-date the event. There is, of course, a risk of supposing two movements to correspond which are really due to different sources, but this risk, though possibly reduced, cannot be wholly avoided even if we confine our attention to one and the same element at the two stations.

A peculiarity of Dr. Bauer's theory requires to be mentioned here to explain Mr. Faris' method of procedure. It was suggested recently by Prof. Kr. Birkeland * that "sudden commencements" are due to an overhead current in the magnetic equator. Birkeland had observed that these movements are usually largest in the H trace, and he believed them to be normally largest in equatorial regions. At the same time, the amplitude was admitted by Birkeland to be considerable even in temperate latitudes, and to account for this he recognised that the hypothetical equatorial current must be at a great height above the ground, a height, in fact, comparable with the earth's radius. Dr. Bauer has examined Prof. Birkeland's theory and has come to the conclusion that it is not correct as regards the height of the current, and that while an overhead current is the true explanation, its height is only of the order of 100 km., and its plane is that of the geographical, not the magnetic, equator. I myself some time ago, when discussing the magnetic results of the National Antarctic Expedition † of 1901–4, pointed out that the "sudden commencements" apparent in the Antarctic curves seemed incompatible with the truth of Birkeland's theory. They seem, however, still less compatible with the truth of Bauer's theory. This theory, moreover, seems to me unsatisfactory in several respects, but this may be my fault, as I am unable to follow the mathematical calculations. Dr. Bauer seems to suppose an ion or group of ions suddenly to commence describing a circular orbit in the earth's equator, making a complete revolution in from three to four minutes, and apparently he supposes that all points on the same geographical meridian experience a commencing disturbance simultaneously. Presumably, the disturbance commences when the leading ion reaches the meridian in question. The E.M.F. which Dr. Bauer associates with the ionic movement seems to be that of the earth's potential gradient, and apparently he regards the field as having the same strength—100 volts per metre—as at the ground level. This I cannot follow at all, because a vertical field seems incapable of explaining a horizontal current, and, so far as we know, the earth's electric field practically disappears within a few miles of the earth's surface. Then even if some object, say a satellite, carrying an electric charge, went round the earth in the equatorial plane, completing a revolution in $3\frac{3}{4}$ minutes, one would expect electromagnetic waves to start from the satellite at every instant and to travel with a velocity equal or similar to that of light, spreading out in all directions, and not merely in the meridian plane containing the satellite at the moment. Thus some disturbance ought to be felt at every spot on the earth's surface practically as soon as the satellite's movement began, and one would not expect any marked maximum of disturbance when the satellite reached the meridian containing the station, unless the satellite passed nearly overhead. Thus I fail to see why, even if a satellite existed, a disturbance should travel round the earth in one direction only; and supposing such a peculiar form of propagation possible, there is no obvious reason why it should not circumnavigate the earth several times. The amplitude of the disturbance is often of the same order at stations such as Kew, Bombay and Christchurch, N.Z., so that a complete circuit of the earth ought, one would think, to be far from sufficient to reduce the disturbance to vanishing point. Thus what one would expect if a comparatively slow rate of transmission exists is a *succession* of wave-like movements, crests and hollows following one another at nearly regular intervals. This type of phenomenon is, as a matter of fact, not at all an unusual one, the interval between successive crests varying on different occasions from several minutes to a few seconds. But this is essentially *not* typical of "sudden" commencements, at least as recorded in temperate latitudes. At Kew, for instance, what one usually does see in the H trace is a rise lasting four or five minutes, sometimes preceded by a relatively insignificant fall lasting a minute or two. The rise seldom goes on for more than 10 minutes, but the element usually retains an enhanced value for at least 15 or 20 minutes, and usually there is no obvious repetition of the commencing movement.

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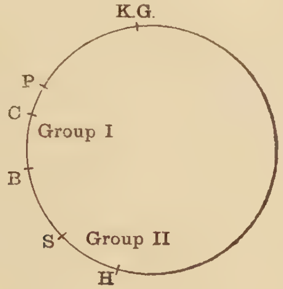
* The Norwegian Aurora Polaris Expedition, 1902–1903, Vol. I., pp. 78, *et seq.*

† *L.c.*, p. 274. *See* also "Nature," May 19, 1910.

To return to Mr. Faris' paper: following Dr. Bauer, he regards a disturbance as travelling *either* eastward or westward, appearing simultaneously at all points in the same meridian. We may thus regard his five stations P (Porto Rico), C (Cheltenham, U.S.), B (Baldwin), S (Sitka) and H (Honolulu) as situated on the same circle, at distances corresponding to their difference of longitude from Greenwich (G), with which we may regard Kew (K) as coincident. Mr. Faris splits his stations into two groups, P, C and B being included in Group I., and S and H in Group II. He takes a mean time of commencement for the Group I. stations and a mean time for the Group II. stations, and regards the difference between the two as representing the time taken by the disturbance to travel round the circle over the arc intervening between the two imaginary stations which represent the centres of the two groups.



If the disturbance be assumed to travel counter-clockwise—*i.e.*, to the west—and the time of commencement at Group I. is the earlier, then the disturbance must have originated somewhere in the long arc between H and P, and the Group II. time will follow the Group I. time by the time required to travel 68° (the short arc between the imaginary centres of the two groups). If, however, still supposing a westerly propagation, the Group II. time is the earlier, then the disturbance must be supposed to originate in the short arc between B and S, and the time interval between the imaginary central stations must represent that required to go through 360°–68°, or 292°. With disturbances assumed to be travelling eastward, the origin must be regarded as in the long arc between H and P, or in the short arc between B and S, according as the time interval between the imaginary central stations is supposed to answer to an arc of 68° or to one of 292°. As the long arc from H to P is 267°, while the short arc from B to S is only 40°, one would expect only a small minority of storms to originate in the short arc ; it is thus somewhat surprising to find that the majority of Mr. Faris' storms—in fact, 9 out of 15—must be regarded as originating in the short arc, as they are supposed to take the long route in passing between the imaginary central stations. Another suggestive phenomenon is that the average of the times of completely encircling the earth, calculated from the differences of the times of commencement at the imaginary central stations, is according to Mr. Faris' own figures—taking the mean from his D, H and V results—only 2·9 minutes for the disturbances taking the long route between the central stations, as against 4·3 minutes for those taking the short route. Further, if instead of following Mr. Faris' method, we take the earliest time shown by any trace at a station as the true commencement for that station, the means from the disturbances taking the long and the short routes become respectively 2·8 and 4·9 minutes.

Mr. Faris' reason for splitting his stations into groups is the uncertainty in fixing the exact time of a disturbance on a particular trace. By taking a mean from several stations this uncertainty is reduced. A great deal turns on what this uncertainty actually is. Mr. Faris (*l. c.* p. 99), seems to claim that with " especial care "—*i.e.*, presumably with the care actually exercised in his investigation—times can be scaled to an accuracy " within one half-minute." This means, I think, only that the time at which the centre of the spot of light fell on a particular point of the photographic paper can be determined after the photograph is developed with an error not exceeding $\frac{1}{2}$ minute. This, I may say in passing, is a high standard to maintain in records having the ordinary openness of time scale. The claim may be warranted if interpreted in this way, but not, I think, if the meaning intended is that the true time of commencement of a disturbance can be fixed to within $\frac{1}{2}$ minute. As evidence to the contrary, we have the fact that the times of commencement deduced by Mr. Faris from his H and D curves differed on the average by +0·4 minute—the + sign denoting earlier occurrence in H—individual differences varying from −0·9 to +1·7 minutes, while the corresponding differences in the case of the H and V curves averaged no less than +2·3 minutes.

From the arguments advanced earlier, the difference between the times of commencement derived from the different traces cannot well represent anything but difference between the errors in the times assigned to the actual commencements. An examination of Mr. Faris' individual disturbances points to the same conclusion. In almost every case some of the results appear to be inconsistent with the view that the times given are true measurements of the commencements to within 0·5 minute. If they were so, then the interval between the times at any two stations should not differ from the interval deduced from the rate of propagation calculated by Mr. Faris by more than one minute. A few examples must suffice. Disturbance No. 3 is supposed to be going west, passing the Group II. stations earliest. Thus the time at P—see the figure—should precede that at C; on the contrary, the earliest record at C preceded that at P by 2·8 minutes. Disturbance No. 8 is supposed to be going west, passing first the Group I. stations. Mr. Faris' estimated times of encircling the earth for this disturbance vary from 2·1 to 9·5 minutes. The longitude interval from P to C is 11° 24′, so that the times of commencement should have differed by only 0·3 minute at the most, or, allowing uncertainties of ±0·5 minute, by 1·3 minutes. The actual difference of the earliest commencements given by Mr. Faris is 4·4 minutes. Finally, disturbance No. 10 is supposed to be travelling east from Group II. to Group I. stations. Thus the occurrence at S should precede that at B by the time taken to go 40°—the difference in longitude between S and B; on the contrary, the first record at B precedes that at S by 3·0 minutes.

The order of priority of occurrence at the five Coast and Geodetic stations shows some curious phenomena. When disturbance takes the long route in passing between Group I. and Group II. stations, B should come first or fifth, according as the disturbance travels east or west. As a matter of fact, it is once fifth, but six times second or fourth, and never once first. On the other hand, when the disturbance takes the short route B should always come third. It is, however, only once third, as against five times first or second. In deriving these figures I have taken the earliest time, whether shown by the D, H or V trace. The S times derived in this way are remarkable in that out of 13 occasions—two S records are omitted—S never once came last, while on nine occasions it was either first or second. It ought to have been last on three occasions, on which the disturbance is regarded as travelling in the easterly direction. It would be interesting to know in this connection the amplitude of the complete movements at S as compared to those at the other stations. C showed the opposite tendency to S. From its position C, according to the theory, should have come second on the list in about half the storms, and never last. In reality, on 11 out of the 15 occasions it was either last or next last. I may add that I was able to identify 10 of the 15 disturbances with reasonable certainty on the Kew curves, and on 8 of these 10 occasions the time which I deduced from the Kew curve—keeping myself purposely ignorant of Mr. Faris' exact times—came next to the C time. The Kew time, as the figure shows, should on the theory, have always come between the P and H times, and never next to the C time. I do not, however, claim an accuracy of ±0·5 minute in my measurements.

Dr. Bauer, so far as I can judge, seems persuaded that Mr. Faris' figures demonstrate the truth of his conclusion, that disturbances normally are propagated round the earth, sometimes eastward, sometimes westward, the time of a complete revolution averaging about 3¾ minutes. Whether this is Mr. Faris' own view I feel some doubts. His statement at the foot of p. 103 of the paper already mentioned certainly suggests a belief that he has proved that magnetic disturbances do not begin simultaneously at different places. At the top, however, of his p. 104 he says " the impression of the simultaneity of magnetic storms . . . may now be stated as not proven." A verdict of " not proven " means in Scotland—the only part of this realm where it is, I believe, a recognised legal term—that a jury has held the evidence insufficient to justify a verdict either of " guilty " or " not guilty." If this is what Mr. Faris means, it is a not unreasonable position to adopt.

As regards the future, I think Dr. Bauer's conclusions could be adequately tested by a careful comparison of curves from selected stations fairly encircling the globe, choosing if possible stations whose time measurements are specially reliable. So far as position is concerned, the following would seem an adequate choice: Apia (172°W.), Honolulu (158°W.), Sitka (135°W.), Toronto (79°W.), or Cheltenham (77°W.), Porto Rico (65°W.), two or three of the best stations in western and central Europe, Pavlovsk (30°E.), Tiflis (45°E.), Mauritius (58°E.), Alibag (73°E.), Toungoo (96°E.), Batavia (107°E.), Lukiapang or Manila (each 121°E.), Melbourne (145°E.) and Christchurch, N.Z. (173°E.).

I should also advise that attention be given to the magnitude and direction of the commencing disturbances as well as to their time of occurrence. If they are due to overhead currents, either in the geographical or the magnetic equator, then in arctic or antarctic stations the horizontal component should tend to be small compared to that in equatorial stations, and the vertical component to become dominant. This was not true of the sudden commencements observed at the station of the National Antarctic Expedition in 1902-3. If sudden commencements are propagated in the way and with the velocity Dr. Bauer supposes, the phenomena in high latitudes must be very curious indeed. An observer, for instance, within a few hundred yards of either pole could easily keep pace with the disturbance.

ABSTRACT.

The question of the simultaneity of magnetic disturbances recorded at different stations has recently been discussed by Dr. Bauer and Mr. Faris. A good many magnetic storms have so-called "sudden commencements." As regards these "sudden" changes three things are conceivable: They may be absolutely simultaneous at different stations; there may be a very small difference of time corresponding to the rate of propagation of electromagnetic waves; or, finally, there may be, as Dr. Bauer concludes, longer intervals, amounting to several minutes, for stations remote from one another. Dr. Bauer concludes that Mr. Faris figures demonstrate the truth of his theory that disturbances normally are propagated round the earth, sometimes eastward, sometimes westward, the time of a complete revolution averaging about 3¾ minutes. The author of the present paper discusses the weaknesses of Dr. Bauer's theory. He points out that the theory could be adequately tested only by a careful comparison of curves from selected stations fairly encircling the globe, choosing, if possible, stations whose time measurements are specially reliable.

V. *On Cusped Waves of Light and the Theory of the Rainbow*

By W. B. MORTON, *M.A., Queen's University, Belfast.*

FIRST RECEIVED SEPTEMBER 9, 1910. RECEIVED IN REVISED FORM AND READ NOVEMBER 11, 1910.

Introduction.

THERE is a tendency at the present time to base the teaching of optics on the conception of the wave rather than on that of the ray. In this connection Prof. R. W. Wood [*] has published interesting drawings of the cupsed waves which arise when light is reflected at a spherical mirror. There is another important case in which similar waves occur—viz., the passage of light through a spherical drop of water in the formation of the rainbow. It seemed of some interest to obtain the forms of waves in this case. So far as I know attention has hitherto been directed only to the emergent waves and the imaginary forms obtained by continuing these backwards.[†] In the diagrams 1, 2, 3 which accompany this paper are shown the forms assumed by a wave of light, originally plane, in three successive journeys across the drop, with the corresponding emergent forms. The second and third of the latter are associated with the primary and secondary bows. In connection with the drawings I wish to call attention to some characteristics of these waves, and, in particular, to the advantage of the wave method in giving a simple explanation of the main features of the "supernumerary bows."

Description of the Figures.

The waves are drawn for a succession of intervals of time each equal to an eighth of the time taken to cross the diameter of the drop The numbers attached to the waves show the number of intervals elapsed since the first incidence. In making the drawings the course adopted was to calculate the successive points of incidence of 10 rays falling on the drop at angles of incidence 0°, 10° . . . 90°, and to mark these off by a protractor on a large scale diagram. The points on the wave fronts were then obtained by measuring off a series of equal optical distances along the rays so obtained. The index of refraction was taken as 4/3.

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[*] Wood, "Phil. Mag.," V. 50, p. 148, 1900 ; also in "Physical Optics."

[†] *See* in particular Möbius Abh. Sächs. Ges. d Wiss,Vol. XXX., p. 107, 1907.

The terminal rays which enter and leave the drop tangentially are shown as thin lines. The ends of the waves run along these, while the cusps run along the dotted lines which show the parts of the caustic curves lying in the region of the actual waves. Prof. Wood, in calling attention (*loc. cit.*) to this property of the caustic, remarks that he had been unable to find any previous mention of it, so perhaps an historical note may not be out of place here. The property was enunciated in a paper read by R. Potter[*] before the British Association at its third meeting, at Cambridge, in 1833. The statement will be found in a paper by Potter in the Cambridge Transactions for 1836, with the title "Mathematical Considerations on the Problem of the Rainbow, showing it to belong to Physical Optics." Further on in the same volume is Airy's classical investigation of the same subject. The fact that the caustic is the locus of cusps on the waves is recognised also in later papers by Jamin[†] and Macé de Lepinay[‡].

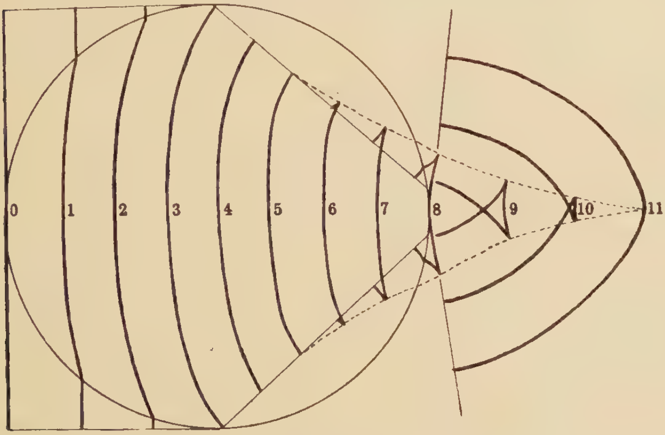

FIG. 1.

Returning to the diagrams one rather curious point may be noticed in connection with the progressive change of form of the surfaces. At certain epochs the original flat circular portion punched out of the incident wave is crumpled up into a com-

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[*] Potter, Trans. of Camb. Phil. Soc., Vol. VI., p. 141., 1836.
[†] Jamin, "Comptes Rendus," Vol. LXVI., p. 938, 1868.
[‡] Macé de Lepinay, "Journ. de Phys," Vol. VII., p. 209, 1898.

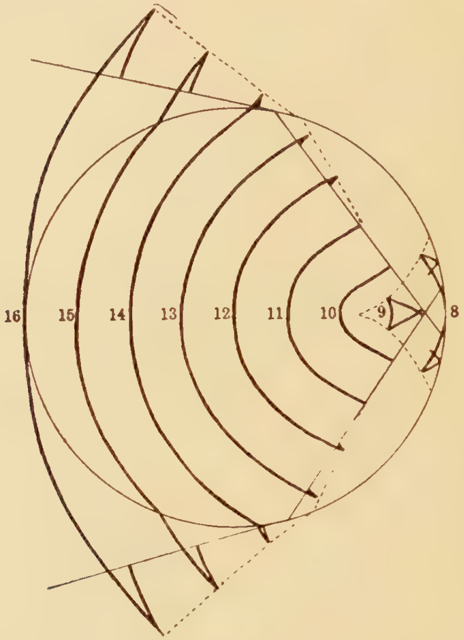

FIG. 2.

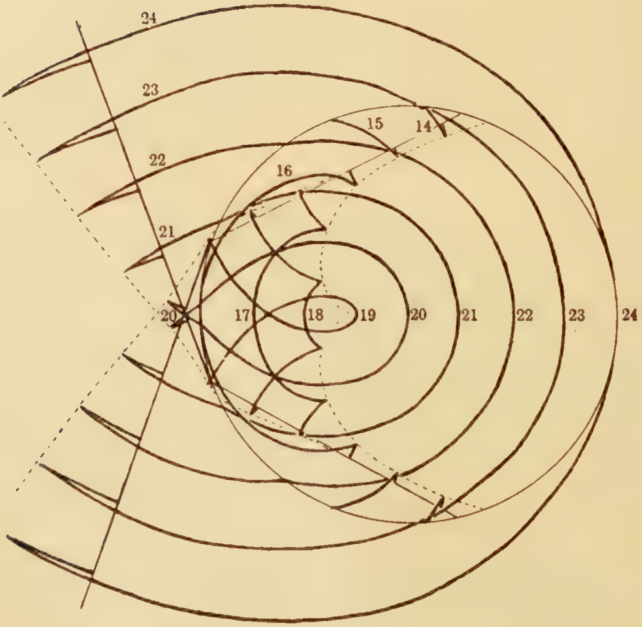

FIG. 3.

pletely closed surface. This happens when the two terminal rays cross on the axis—*e.g.*, a little before epoch 9 on Fig. 2.

The Emergent Light.

The emergent caustics in Figs. 2 and 3 have as asymptotes the rays of minimum deviation. At a short distance from the drop the caustics practically coincide with the rays in question. The cusps of the waves run along this line and the energy, being condensed near the cusps, is guided along this direction without appreciable diminution, giving the concentration of brightness observed in the bow. For comparison the waves passing out at the back of the drop are shown in Fig. 1 for epochs 9, 10 and 11. It will be seen that the emergent beam of light is now bounded by the terminal rays instead of by the asymptotes of the caustic.

It is at once obvious that the branches of consecutive waves in the neighbourhood of the cusps will intersect, and that interference effects will thus be produced. This explains the occurrence of the maxima and minima of brightness seen in the so-called supernumerary bows. For the purpose of an elementary physical explanation it seems more natural to appeal to the peculiarities of the waves which enter the eye rather than to the form of an imaginary wave surface in the drop, as is done in Mascart's method. We shall see that very simple geometrical considerations show the identity of the results obtained by the two methods.

The idea of the interference of overlapping cusped waves was used by Potter, Jamin and Macé de Lepinay in the papers already referred to. The results obtained by Potter and Jamin were vitiated by the assumption that the phase on the two branches of a wave was the same. Macé de Lepinay investigated analytically the distribution of intensity in the focal plane of a telescope receiving the waves and showed his formulæ to agree with Mascart's.

Phase Relations on the Waves.

The first point to notice in considering the interference of cusped waves is that these do not fulfil the ordinary definition of a wave as a surface over which the disturbance is in the same phase. In passing through a centre of curvature of the wave surface—*i.e.*, through a point of contact of a ray with the caustic the disturbance receives a sudden advance of phase amounting to a quarter period.* When the two centres of curvature coincide, as in the case of a homocentric pencil, the sudden change amounts to a half period, or reversal of phase. What appears to be the corresponding effect in a sound wave may be seen in some of the photographs in which Prof. Wood has obtained exact analogies of optical phenomena. Now, if we consider the two branches of a wave near a cusp it is easy to see that the rays normal to one branch have already passed their contact with the caustic, while those normal to the other branch are still approaching the caustic. Therefore, there is a difference of a quarter period between the phases on the two branches which must be taken into account when investigating the interference due to the crossing of the opposite branches of consecutive waves. For darkness we must have the distance between the branches of a wave equal to $(2n+1)\frac{\lambda}{2}+\frac{\lambda}{4}$.

In this connection it is interesting to notice that the distribution of phases over some of the wave-forms shown in the figures accompanying this paper will be different according as the curves are regarded as sections of cylindrical waves or of surfaces of revolution. In the latter case one of the centres of curvature at any point of a wave lies on the axis ; in the former case one centre of curvature is at infinity. So in the case of a wave which is a surface of revolution an advance of a quarter period occurs when the wave crosses the axis, at a node on the plane section. Instances of this are seen in the internal and external waves numbered 9 in diagrams 1 and 2. If the wave is cylindrical there are only two phases over it, the advancing front being a quarter period behind the rest of the wave. But if the diagram represents a spherical drop there are three different phases, the front part being a quarter period behind the portions between the cusps and the node, and these again the same amount behind the part between the node and the ends. Of course, in the wave the cusps of the plane section are on a ring-shaped edge and the node is a conical point. It is interesting to follow the evolution of these waves into the succeeding forms and to see how the phases become once more equalised as the curve becomes free from singularities.

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* *Cf.* Walker, "Analytical Theory of Light," p. 91, where a reference is given to Poincaré's "Theorie de la lumiére." For an account of recent theory and experiment on this point *see* a paper by W. Möbius, "Ann. d. Phys.," Vol. XXXIII., p. 79, 1910. In the immediate neighbourhood of the focus there are rapid oscillations of phase, so that in the above discussion we exclude a small anomalous region very close to the cusp.

Application to the Case of the Rainbow.

In order to examine the conditions determining the intensity of illumination in a given direction, suppose an eye, adapted for parallel light, to look in the direction A′P or B′Q (Fig. 4). Let AB′ be a plane perpendicular to this direction. ACB and A′C′B′ are two waves such that the A branch of the first and the B branch of the second both touch the plane AB′. Then the disturbances from the elements at A and B′ will reach a point on the retina together. The geometrical distance between the two waves is AA′ or BB′, but taking into account the difference of phase between the two branches of a wave the optical path-difference becomes $BB' - \frac{\lambda}{4}$. Ignoring any difference of intensity between the elements A and B′ we find as a first approximation that there will be maximum intensity if $BB' = n\lambda + \frac{\lambda}{4}$, minimum if $BB' = (2n+1)\frac{\lambda}{2} + \frac{\lambda}{4}$. BB′ is the distance between the parallel tangent planes drawn to the two branches of one wave—*e.g.*, the tangent planes at the points A and B. It evidently corresponds to the spacing of the branches at a large distance from the drop.

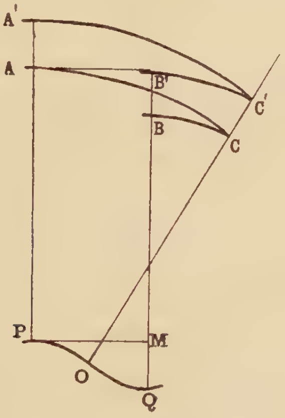

FIG. 4.

Comparison with Mascart's Method.

Mascart, following Airy, supposes the emergent waves continued backwards in a series of parallel surfaces, till a specially simple form, without cusps, is reached. All the waves have points of inflection on the asymptote to the caustic.* This special surface is so placed that the tangent at the inflection on its section passes through the centre of the drop. In the neighbourhood of the inflection the form of the section approximates to that of a cubical parabola. It is represented by POQ on Fig. 4, O being the point of inflection and OCC′ the asymptote to the caustic. The figure is, of course, not drawn to scale. Airy's exact solution was obtained by integrating over this special surface in accordance with Huygens' principle. Mascart showed that an approximation to the correct result could be got by supposing the effect in a direction, such as PA or QB, to be due to the interference of disturbances sent from sources at the points P and Q, the poles of the wave corresponding to this direction. The geometrical path difference is MQ. From this $\frac{\lambda}{4}$ is deducted because the wave is convex at one pole and concave at the other, a statement which is equivalent in effect to recognition of the change of phase in passing a centre of curvature. Now, the curves POQ and ACB are parallel, therefore,

$$AP=BQ,$$
$$B'M=BQ,$$
$$BB'=MQ,$$

so we reach the same result as before.

ABSTRACT.

Diagrams were shown of the forms assumed by a plane wave of light falling on a spherical raindrop and twice reflected from the interior of the drop, as well as the waves emerging from the drop. The waves in general have cuspidal edges which run along the caustic surfaces. This relation between the caustic and the cusps on the waves was pointed out by Wood in connection with the similar waves produced by reflection at a spherical surface. It had been noticed earlier by Potter, Jamin and Macé de Lepinay. The phase over a wave of this type is not constant, the two portions on opposite sides of a cusp differing in general by a quarter period. Attention was drawn to the advantage of regarding the distribution of light in the rainbow as the consequence of the interference of the cusped waves which run down to the observer's eye along the direction of minimum deviation. This way of looking at the matter is shown to be equivalent to Mascart's approximate method of explana-

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* It is the reversal of curvature on crossing the asymptote which makes the two branches of the emergent waves appear concave in the same direction. The portion between asymptote and caustic, where the branches are oppositely curved, is too narrow to show on the diagram.

tion of the formation of the supernumerary bows by interference of disturbances coming from the two poles on the special wave-form used by Airy.

DISCUSSION.

Prof. C. H. **Lees** pointed out that the boundary of the disturbance as it passed through the drop would be subject to diffraction effects. He asked if this would make any material difference in the results arrived at in the paper.

The **Author**, in reply, stated that diffraction phenomena would not seriously affect the diagrams he had obtained.

VI. *The Afterglow of Electric Discharge.*

By The Hon. R. J. STRUTT, *F.R.S., Professor of Physics, Imperial College of Science, South Kensington.*

RECEIVED AND READ NOVEMBER 25, 1910.

Section I.—*Introduction.*

IT is quite 50 years since Geissler first observed the afterglow which occurs in vacuum tubes containing particular gaseous mixtures. A considerable number of papers have appeared since then containing discussions of the phenomenon, and contributions to its experimental study. A summary will be found in Kayser's "Handbuch der Spectroscopie," Vol. I., p. 249. In addition to the papers mentioned by him reference should be made to two of special interest, *i.e.*, Newall, Proc. Camb. Phil. Soc., IX., p. 295, and Dewar, Proc. Roy. Inst., 1888.

In the latter paper a most valuable and ingenious method of studying the effect is introduced. A powerful air pump is used to draw a regulated current of gas through the vacuum tube. In this way a continuous removal of the gas from the region of discharge is effected, and the afterglow which it emits in passing through another vessel on its way to the pump can be examined continuously and at leisure.

Dr. Gaede's new mechanical rotatory pump (not the mercury pump) has been found very convenient for repeating this experiment on a moderate scale.

There has been considerable difference of opinion as to whether pure oxygen shows a glow or not. Under the conditions of my experiments the glow, if any, is certainly exceedingly faint. With air, on the other hand, a bright yellow glow is obtained, which is improved by enriching the air with oxygen. This is necessarily an optimum mixture, but I have not determined its exact composition. It has no particular significance, as will appear later. Pure nitrogen gives no glow whatever. The glow from air is not, in my experience, appreciably affected by drying the air, or by saturating it with moisture.

Section II.—*Connection of the Glow with Ozone.*

Several previous experimenters have connected the glow with ozone, though generally without expressing any very definite view as to what part ozone played in its production. The following experiments convinced me of the correctness of their opinion.

(1) The tube carrying the glowing gas was heated to about 300° C.; the glow was extinguished and did not revive to any considerable extent, as the same gas was carried on to a colder part of the tube.* This, a repetition in modified form of previous experiments by Dewar and J. J. Thomson, agrees well with the hypothesis.

(2) The glow is not able to survive passage through a tube cooled in liquid air. I regard this as due to condensation of ozone.

(3) The glow is destroyed by passage over oxides of copper or manganese, in the cold. Ozone, too, is known to be destroyed by these substances. The glow can, to a great extent, survive passage through glass wool, though brought into far more intimate contact with it than with the above oxides, which were in large pieces.

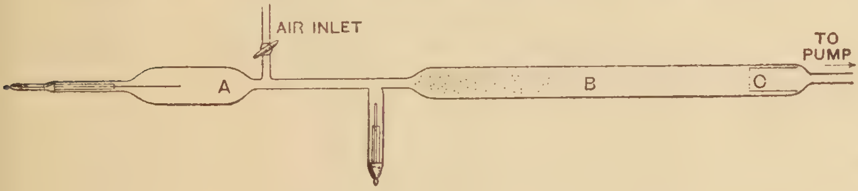

FIG. 1.

(4) Silver gauze is oxidised in the glow to a dead black oxide, and the gauze thus oxidised absolutely destroys the after luminosity of air passing through it. One single layer of the silver gauze is sufficient. A similar partition of iron gauze has little effect.

The apparatus shown in Fig. 1 may be used to exhibit this action of silver. Glowing gas from a vacuum tube, A, passes into the cylindrical vessel B, remaining luminous through most of the length. B contains a partition consisting of one layer of silver gauze, C, carried in a short length of glass tubing, and able easily to slide in the vessel. If the silver partition is moved towards A by tipping the tube, it will be found to cut off the glow completely, although the gas, deprived of glow, of course continues to pass through it. This is a striking experiment. The prejudicial effect of silver on the glow was observed long ago by Sarasin ("Arch. des sc. Phys. et Nat." (2) 34. p. 243), but the conditions of his experiments were, as Kayser remarks, unsatisfactory and the result scarcely convincing.

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* There was, however, a slight recovery which is difficult to account for.

(5) It was found that silver remains bright for any length of time if exposed to the gas current beyond the point at which the glow had died out. This suggests that the cessation of luminosity is conditioned by all the ozone being used up.

(6) The following experiment is confirmatory of this. paper impregnated with thallium hydroxide is rapidly turned brown in the glow, showing that ozone is present in it. Beyond the glow, no ozone can be detected by this test.

(7) Dewar (*loc. cit.*) has observed that hydrocarbons introduced into the discharge destroy the glow. Air mixed with a small proportion of benzole vapour was led into the discharge; although the spectrum still showed little but nitrogen bands, the after glow had disappeared. Silver placed in the issuing gas now remained quite bright. It appears that the action of a hydrocarbon is to prevent the delivery of any ozone. It is probably used up in oxidising the hydrocarbon.

Section III.—*Nature of the Glow.*

The experiments so far recorded show that the glow involves consumption of ozone. It is therefore natural to regard it as a flame of low temperature, arising from the oxidation of some other body by ozone. The next series of experiments were directed to determining the nature of the other body. Ozone was produced in a vacuum tube, through which a current of rarified oxygen passed, in itself producing little or no after-glow. The ozone thus produced was allowed to mix, in a Y-shaped tube, with any other gas which it was desired to test, on its way to the pump.

(1) No effect was observed on mixing the ozone with nitrogen, whether the latter had just passed through a vacuum discharge tube or not.

(2) The second branch of the Y-tube (Fig. 2) was next supplied with a current of air which had just passed through a second vacuum discharge tube, B, quite independent of the first one, A, in which the ozone was produced. This air current was deprived of glow by passing through a silver gauge partition, C, before meeting the ozone current from the other tube. Each of the confluent streams was by itself dark. The

characteristic glow was produced at the point of confluence, and it disappeared if the discharge was stopped in either tube. It may be inferred, then, that air after passage through the discharge contains some constituent which can be oxidised by ozone, producing the glow. This constituent could not well be anything but one of the lower oxides of nitrogen, produced by the discharge.

(3) No effect was produced by mixing nitrous oxide with the stream of ozone, but with nitric oxide (NO) a glow was obtained in the Y-tube of the characteristic yellow colour, and far brighter than anything that had been seen in the previous experiments. It will be understood that no discharge was passed through the nitric oxide, which was merely fed in as a tributary to the stream of oxygen which had been through a discharge.

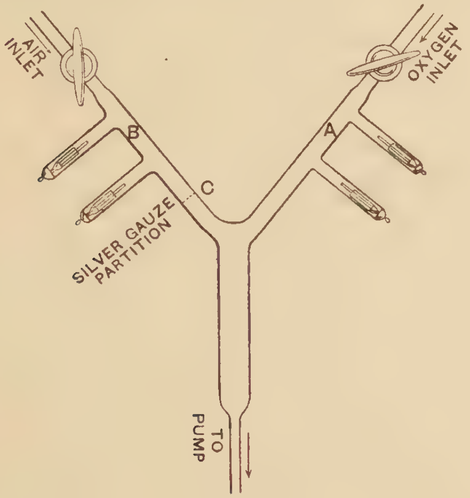

FIG. 2.

The apparatus shown in Fig. 3, serves well to show the experiment to an audience. Ozone is produced at a low pressure in the vacuum tube A. A current of it passes through the jet B into the vessel C, for which a separating funnel, or rather a separating cylinder, of the largest size, answers well. A gentle current of nitric oxide is fed in through D, and enters C by an annular opening concentric with B. The gases are led out to the pump through E. With a small ozone feed, a very bright glow of almost spherical form surrounded by a misty halo may be seen issuing from the jet. If the oxygen feed is increased the glow gradually takes on the form of a pointed flame, similar in shape to that of the oxyhydrogen blowpipe, and showing a well-defined inner cone of small luminosity.

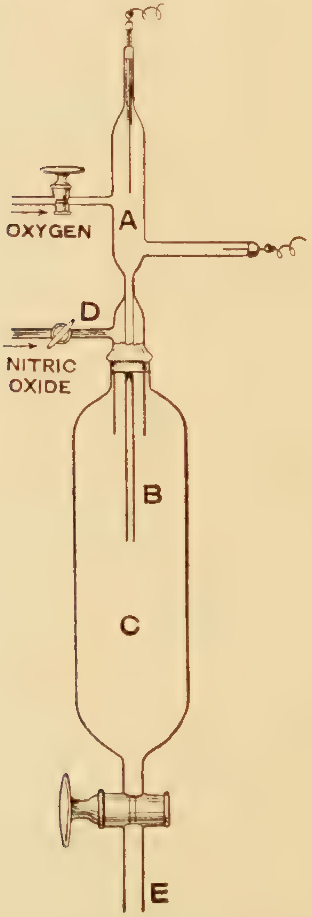

Fig. 3.

I conclude, then, that the afterglow results from these three distinct processes:

(*a*) Formation of ozone in the discharge;

(*b*) Formation of nitric oxide in the discharge,

(*c*) Oxidation of nitric oxide by ozone, after the discharge is over.

Section IV.—*Further Experiments.*

It was proved by the above experiments that ozone straight from a vacuum discharge was capable of producing the glow. An experiment was tried to determine whether the residual ionisation of the oxygen had any connection with the glow. A stream of oxygen from the discharge was passed for three or four minutes through a U-tube (A, Fig. 4), cooled in liquid air, to condense the ozone produced. This U-tube was then shut off by stopcocks B and C, and allowed to warm up. On admitting a small volume of nitric oxide from the space between the taps D and E, a momentary flash of the characteristic yellow colour was observed. Ionisation by the discharge is thus shown not to be directly concerned, for the glow can be obtained long after the discharge is over, and the ions have recombined.

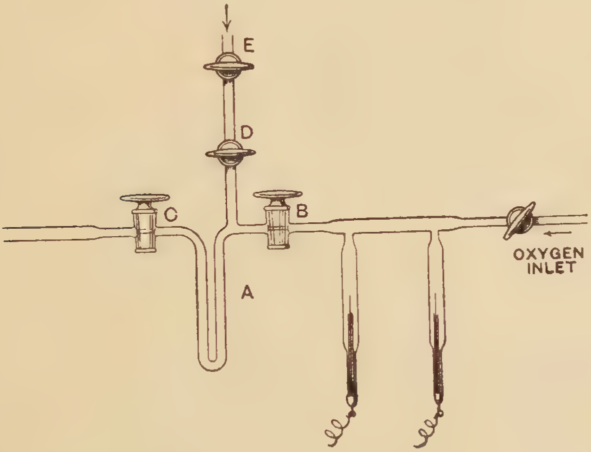

Fig. 4.

I have not been able to obtain the glow by leading oxygen at atmospheric pressure through a Siemens ozone tube, and then into nitric oxide. On reducing the pressure to a few millimetres the glow is obtained, but much less successfully than with the vacuum tube. I am not yet in a position to explain this, but the point will be investigated further.

The temperature of the glow is certainly very low. I have not been able to detect any rise of temperature with certainty, but accurate observations are difficult, since the gas stream is heated several degrees in passing through the low-pressure discharge. The rise of temperature due to the glow itself was found to be certainly less than $\frac{1}{2}$° C., under the particular pressure conditions of the experiment.

The spectrum of the nitric oxide glow has been photographed with a small quartz spectrograph, with the kind assistance of Mr. Eagle. It was found to be continuous, and to extend from λ4,200—λ6,700. It doubtless extends beyond that into the infra red. The continuous character of the spectrum confirms Newall's observations.

Section V.—*Conclusion and Summary.*

It is proposed to study the action of ozone on other easily oxidisable gases. This will probably reveal the origin of various other kinds of afterglow. For instance, the blue one obtained by Newall (*loc. cit.*) in mixtures containing oxygen and sulphur. It is hoped also to study more closely the electrical and other properties of these ozone-supported flames, which have probably a far lower temperature than any others equally bright.

The main conclusion of this paper is that the ordinary yellow afterglow is due to oxidation of nitric oxide by ozone.

NOTE ADDED DECEMBER 20TH.—It should have been mentioned that Sir James Dewar returned to the subject of the afterglow at the Royal Institution in the summer of 1909 with additional illustrations. (*See* "Engineering," June 18th, 1909.) He has, I understand, pursued the subject since, and has arrived independently at some of the present results.

ABSTRACT.

It has long been known that when the electric discharge has passed at low pressure through certain gaseous mixtures a luminosity survives for some seconds after the discharge has been turned off. A greatly improved method of experimenting on the phenomenon was introduced by Dewar ("Proc." Roy. Inst., 1888). A powerful air pump is used to draw a regulated current of gas through the vacuum tube. In this way a continuous removal of the gas from the region of discharge is effected

and the afterglow which it emits in passing through another vessel on its way to the pump can be examined continuously and at leisure.

There has been considerable difference of opinion as to whether pure oxygen shows a glow or not. Under the conditions of the author's experiments, the glow, if any, is certainly exceedingly faint. With air, on the other hand, a bright yellow glow is obtained, which is improved by enriching the air with oxygen. Pure nitrogen gives no glow whatever.

Several previous experimenters have connected the glow with ozone, though generally without expressing any very definite view as to what part ozone played in its production. The chief evidence for this hitherto has been that the glow is only obtained where oxygen is present, and that it is destroyed by heat. The following additional evidence was obtained.

1. The glow cannot survive passage through a tube cooled in liquid air. This is regarded as due to condensation of ozone.

2. It is destroyed by passage over oxides of copper, manganese and silver. Ozone, too, is known to be destroyed by these substances.

3. While the glowing gas oxidises bright silver, the gas current beyond the point at which the glow has died out does not do so. Disappearance of the glow is simultaneous with disappearance of ozone from the gas.

The glow is thus shown to involve consumption of ozone. It is, therefore, natural to regard it as a flame of low temperature, arising from the oxidation of some other body by ozone. The next experiments were made to determine the nature of this other body. A current of ozone from a vacuum tube fed with oxygen was allowed to mix, in a Y-shaped tube, with any other gas which it was desired to test, on its way to the pump.

Nitrogen or ordinary air added to the ozone gave no effect, but air, which had been through an independent discharge, and had been deprived of its original glow by silver oxide, was found to glow again on mixing with ozone. Clearly then some body is produced in air by the discharge whose oxidation is responsible for the glow.

This body proves to be nitric oxide. On leading a current of this gas into the ozone stream a very brilliant glow was obtained, of the characteristic yellow colour. By suitable arrangements this glow can be produced in the form of a pointed flame, with dark inner cone. The glow is not associated with a sensible rise of temperature. Condensing the ozone with liquid air, allowing it to re-evaporate, and admitting nitric oxide to it, a yellow flash can be obtained, long after the electric discharge is over. The glow is purely chemical in its origin.

Ozone from the Siemens tube used at atmospheric pressure seems incapable of yielding the glow when mixed with nitric oxide. This may be due to the low percentage of ozone present. Further investigation is in progress on this point, and also on the possible production of a glow in other cases of oxidation by ozone.

The main conclusion of the present paper is that the ordinary yellow afterglow is due to oxidation of nitric oxide by ozone.

DISCUSSION.

Dr. J. A. Harker asked if the presence or absence of water vapour had any effect upon the results obtained.

The Author replied that the same results were obtained with air saturated with water vapour and air dried by means of phosphoric anhydride.

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